THE

Medical and Phyſical Journal.

VOL. VII.] APRIL, 1802. [NO. XXXVIII.

To Dr. BRADLEY.

DEAR SIR,

I Send you, for your valuable Publication, the plate of a machine for conveying a vapour bath to diſeaſed limbs, and for taking off the preſſure of the atmoſphere; which, if I am not deceived in its effects, is calculated to prove of the greateſt uſe in changing the action of diſeaſed parts, little doubting but it will rank among the firſt of the modern improvements in medicine, and, under the ſuperintendance of profeſſional men, prove a powerful means not only of alleviating, but frequently curing, many diſeaſes which have been conſidered incurable.

As the machine comprehends, and, for the firſt time, combines the application of fomentation and the cupping-glaſs, two of the moſt powerful external means of acting on diſeaſe, and that on a plan more extenſive than was ever before thought of; what effects may we not be led to expect from ſo extended a combination? Every day's experience is proving to us the efficacy of the application in gout, rheumatiſm, contractions of muſcles, cutaneous and other diſeaſes, particularly of the chronic kind; and though its application has not yet been ſo extenſive, under my own immediate inſpection, as to warrant my affirming it to the extent I hope to be ſoon able to do; nevertheleſs I have reaſon to believe, from ſome recent caſes, that acute diſeaſes alſo are not beyond the reach of its influence, and that from its power in promoting a copious diaphoreſis and relaxation of the whole ſyſtem, it may be made to produce the happieſt effects in phrenitis, pleuritis, enteritis, &c.

The peculiar conſtruction of the machine before us is adapted to the leg or arm only, but the principle extends much further, and can be applied to any part of the body by the intervention of glaſſes, or otherwiſe. One is already made that includes one half of the body; and when we conſider that by its means the preſſure of the atmoſphere can be removed from ſo large a ſurface, every ſquare inch of which ſuſtains a weight of 15 lb. the effects on the veſſels circulating the fluids in ſuch parts are too evident to need inſiſting on in this Work. One

very obvious one is, that the confequent tempo[illegible] expanfion of them from fo great a preffure being removed, [illegible]ft give an opportunity for obftructions formed in them to [illegible]ve way, at the fame time arrefting the progrefs of inflammation, the means which Nature, left to herfelf, is under the neceffity of employing to bring about the fame end. In this manner is avoided a great deal of mifery arifing from fymp[illegible]atic fever, &c. and not unfrequently deftruction of parts, [illegible]fequence of fuppuration.

Though this is the firft drawing th[illegible] has been made of it, or the firft time any thing has appear[illegible] concerning it in print, yet the credit of it is by no means to eftablifh. It is too well founded in philofophy and good theory not to have attracted the attention of medical men, fome of whom, high in their profeffion, have not only recommended it to their patients, but have had it applied to their own perfons with the moft marked benefit. Many valuable cafes are already in my poffeffion, fome of them of perfonages of the firft confideration in this kingdom; but as it is my intention to publifh an account of them, I fhall content myfelf at prefent w[illegible] fimply ftating its principles, and leave it with your numerous readers to draw their own conclufions.

EXPLANATION of the PLATE.

Fig. 1. A view of that end of the machine to which the exhaufter, &c. are affixed. (A) the body of the machine. (B) the boiler containing the fluid, the fumes of which are thrown into the machine through the ftop-cock (H), and heated by means of (C) a fpirit lamp. (D) the valve in the boiler for the efcape of the fumes when prevented entering into the machine by turning the ftop-cock (H). (E) a thermometer, fhewing the temperature of the fumigation. (F) the exhaufter for exhaufting the machine after the fumigation has continued a proper time, 40 ftrokes of which are as many as any perfon can bear at once. (G) the efcape valve of the exhaufter; to which a flexible pipe is adapted to convey the air from the chamber, if vitiated by the nature of the affection for which the application is made. (H) the ftop-cock. (I) another, to prevent the valves of the exhaufter from being injured by the hot fumes as they enter the machine during the fumigation.

Fig. 2. A perfpective view of the machine from the other end, where the limb is admitted. (K) The body of the machine made of ftrong copper tinned in the infide, tin itfelf not being equal in all cafes to the preffure of the atmofphere. (L) The mouth of the machine, to which is attached a ftrong bladder, cut fo that one end of it may go over, and be firmly fecured to it by means of a ligature, the other fo that it may be drawn over the thigh, when paffed through it into the machine, and fecured upon it by means of a roller.

Mr.

Mr. Sm[illegible] of Brighton, a Member of the Society of Arts, whofe mech[illegible]cal genius is well known to the world, claims t[illegible] merit o[illegible] [illegible]s invention, and has, by occafional alterations in the fpace of fome years, brought it to its prefent ftate of fimplic[illegible]y and p[illegible]. The idea of fucking poifon from wounds firft led him to bel[illegible]e that mechanical means might be employed on the fa[illegible]e pri[illegible]ciple to a [illegible]at extent. How far the intention is anfwere[illegible] by the air pump vapour bath, and how extenfive the application of it m[illegible]y be made in the treatment of difeafe, the public are to [illegible] judges.

I fhall add nothing more at prefent than a letter to the Proprietor from Dr. Hamilton, Phyfician to the London Difpenfary, who accidentally faw the effects produced by its application.

"Dear Sir,

"I have often, fince th[illegible] time I examined your machine for conveying a vapour bath to difeafed limbs, and for taking off the preffure of the atmofphere, confidered it and its probable effect with much atte[illegible]n; and my opinion is, that it is likely to be of the great[illegible]t ufe to mankind, in helping obftructed veffels to unloa[illegible] themfelves. The mode in which it muft act is agreeable to found theory, and the more it is put in practice the oftener wi[illegible] facts occur to confirm what I now fay. I not only view it as valuable to remove local affections of the extremities, but alfo to afford relief in other parts of the body, where obftructions may have taken place. There are fome complaints which would often be greatly mitigated, if not always removed, by its power in producing a general and copious fweat over the whole frame, fuch as in the diabetes, dropfy, and other maladies where the fkin is parched, dry, and hard. In general, it will be well to pump out the air gradually, and to fee the effects of a partial exhauftion of the machine; for its being done too fuddenly, would allow the air in the circulating fluids to exert its fpring with too great force, producing acute pain, &c. The machine, by having a condenfing pump fixed to it, might be ufed as a bracer, and that with beneficial and powerful effects. Was I not fo far advanced in life, and fo much engaged in other avocations, I would with gladnefs have devoted my time to the ufe of it, thoroughly convinced that it would prove ufeful to the diftreffed, and honourable to myfelf. Wifhing you, dear Sir, the favour of God, which is better than life and all that life contains, I am

Your friend, and obedient fervant,
JAMES HAMILTON."

No. 7, Artillery Place,
April 8, 1801.

Profeffional

Profeſſional Gentlemen and others, who have made Philoſophy an object in their reſearches, will have an opportunity of inſpecting the machine itſelf, at No. 330, Oxford Street, and at Mr. James's, No. 5, Cumberland Place, New Road, Mary-le-Bone. I am, &c.

RALPH BLEGBOROUGH,
Member of the Royal College of Surgeons.

Oxford Street, Feb. 10, 1802.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

THE rapid progreſs which Vaccine Inoculation is ſtill making, cannot but afford the moſt ſincere ſatisfaction to every benevolent mind. Advices are received from Turkey, that an infant of the favourite of the Grand Segnior has undergone this ſalutary proceſs. From Ruſſia we hear, that another crowned head alſo patronizes this improvement of the healing art. The Empreſs Dowager has ordered the firſt child who was inoculated with Vaccine virus to be chriſtened Vaccinoff; and ſettled a penſion on her.

I am alſo informed by the Rev. Mr. Pitt, Chaplain to the Engliſh Factory in Ruſſia, that this illuſtrious perſonage countenances the practice. It is a little remarkable, that after repeated failures, the new inoculation ſucceeded at Moſcow while the Court was there; and, at the ſame time, at Peterſburg. From theſe two ſources it is now become general.

Mr. Pitt adds, that although this practice is every where of great importance, it is, perhaps, no where ſo much ſo as in Ruſſia; which he attributes to the habits of the Ruſſian peaſants. A nobleman who has very large eſtates aſſured him, that throughout his villages, the ravages of the Small-pox are little inferior to thoſe of the plague.

To theſe communications on a popular topic I beg leave to add a few others, which, I flatter myſelf, will not prove unintereſting.

It is well known, that in caſes of Ranula, ſimple inciſion frequently effects a cure. In ſome inſtances, however, and particularly where the cyſt is thick, this method is inſufficient.

A caſe of this kind occurred a few months ago; in which a ſurgeon had five times operated in this manner, but without ſucceſs. When the child was brought to me, the tumor was ſo large, that it compreſſed the tongue with great force againſt the

the roof of the mouth, ſo as totally to impede ſucking, and even deglutition. In this extremity, the exciſion of a portion of the cyſt afforded preſent relief, and produced a perfect cure.

The next ſubject on which I ſhall offer a few remarks, is the treatment of the Itch; a diſeaſe which is ſufficiently afflicting and diſguſting, without the aid of that infernal drug, Brimſtone.

Having been frequently diſappointed in my attempts to cure it with ointment of white precipitate, and ſolutions of ſublimate, and having obſerved that the latter cauſed conſiderable inflammation of the ſkin, without proving a certain remedy for the diſtemper, I combined the two with an unctuous ſubſtance; and, from that time, about five and twenty years, have never known it fail in a ſingle inſtance. The form is as follows:

℞. Hydrarg. muriat. gr. x. Calcis hydrarg. alb. ʒj. Adipis ſuill. ℥iij. Eſſent. bergamot. ʒj. M. ft. unguentum.

With this ointment the patients are to rub themſelves all over, every night. The quantity here preſcribed muſt not be uſed in leſs than ten nights, even in an adult. Care muſt be taken, not to rub too much on any particular part, eſpecially on the bend of the elbow; otherwiſe, inflammation and excoriation may be the conſequence.

The abſurd cuſtom of giving ſulphur, or any other medicine, internally, for this cutaneous diſorder, ought to be diſcontinued. I have often known dyſenteries occaſioned by ſuch practices; and the pretended remedy prove worſe than the diſeaſe.

The preparation of mercury here propoſed ſeldom affects the ſalivary glands. Its greateſt inconvenience is a troubleſome heat, raſh, or other effects of too powerful a ſtimulus applied to the ſkin; which, if the patient be properly cautioned, may be inſtantly relieved, by ſubſtituting a little plain lard, inſtead of the ointment. This application ought to be continued, till the troubleſome ſymptoms are perfectly removed.

A preparation of the ſame kind, but only of half the ſtrength of that already preſcribed, is one of the moſt efficacious remedies for the Tinea Capitis; in the cure of which, conſtant care muſt be taken to keep the ſurrounding hair as ſhort as poſſible. By means of this ointment, applied once a day, I have ſucceeded in the cure of every caſe in which it has been tried, amounting to many hundreds. One of the patients had laboured under the diſeaſe fourteen years; and his father wiſely dertermined to get him cured, if poſſible, before he put him out as an apprentice.

Many others have been cured by the ſame remedy, who had been given up as incurable. No internal medicine whatever

was

was exhibited in any of theſe caſes; a ſufficient proof that no internal medicine is neceſſary in ſuch caſes, whatever may be the prejudice, or whatever may be the intereſt, of medical men.

The ſame topical application proves equally efficacious in the caſe of tetters, and various other affections of the ſkin.

Should theſe obſervations be deemed worthy of inſertion, I ſhall ere long add to their number.

New Street, Hanover Square,
March 16, 1802.

I am, &c.
JOHN RING.

Observations on the Cow-Pox, by Dr. WINTERBOTTOM.
Addressed to a Physician in London.

I RETURN you my beſt thanks for the Vaccine matter which you did me the favor of ſending. In upwards of 70 caſes in which I uſed it, it anſwered my warmeſt wiſhes. I ſhall briefly notice to you the reſult of a few of the caſes, as they ſeem to allow of ſome practical inferences being deduced.

On Saturday evening, December 5, 1801, a ſtout healthy child, four years old, belonging to a Captain Park of this town, was ſeized with an epilectic fit, which was ſuppoſed to be a precurſor of the eruption of Small-pox, as the diſeaſe prevailed in the town in a very malignant form; though the family had anxiouſly guarded againſt any immediate expoſure to contagion. In conſequence of this alarm, four children in the ſame family, between the age of ſeven years and eight months, were next morning inoculated with fluid Cow-pock matter taken from a puſtule of the eighth day. On Monday morning the child affected with convulſions had leſs fever, no eruption appeared, and being upon the whole better, though ſtill very dull and inactive, it was inoculated with fluid Vaccine matter taken from the ſame child from which the other had been vaccinated the day before. On Tueſday morning, one of the children that had been inoculated on Sunday was covered with a copious eruption of Variolous puſtules, attended with great fever and reſtleſſneſs. This child, though previouſly very healthy, appeared ſomewhat indiſpoſed on the morning it was inoculated; its ſkin was hotter than uſual, and the cheeks were ſuffuſed with a circular patch of red. On the fifth day, the arms of the children inoculated on Sunday exhibited certain marks of activity; the punctures, two in each arm, appeared ſlightly red, and were beginning to aſſume the indented appearance of the Vaccine puſtule.

puſtule. The child in Small-pox continued to labour under great fever and reſtleſſneſs: The Variolous eruption had greatly increaſed, and continued very ſmall and cluſtered. The inoculated ſpots exactly reſembled thoſe of the other children. On the 8th day, the Vaccine puſtules were very large, and conſiſted of a tumid ring, of a dirty white colour, having in the centre a minute yellow ſcab nearly upon a level with the ring. On the outer part of the ring, a faint bluſh, ſcarcely projecting beyond its circumference, was juſt preceptible. The child in Small-pox continued very ill, the fever ran high; the Variolous puſtules which were ſtill very ſmall had become confluent in many parts, particularly on the face, and in conſequence of pneumonic inflammation, a large bliſter was applied to the cheſt. The Vaccine puſtules were almoſt three times larger than any of the Small-pox, and reſembled thoſe of the other children in every reſpect, except in not having the leaſt appearance of efflorefcence which was alſo wanting at the baſe of the Variolous puſtules. On this day, the child which took the fit, was inoculated a ſecond time with fluid Vaccine matter taken from the arm of the youngeſt child. The firſt inoculation had entirely failed, though the matter ſhewed its activity by cauſing the punctured parts to appear very red and tumid on the ſecond day; the inflammation continued to increaſe until the fourth day more rapidly than in the other four children, during which time ſhe continued liſtleſs and diſpirited, but after this period the punctures ſcabbed over and ſhe regained her uſual ſpirits. On the ninth day the eldeſt child complained of pain in the axillæ; ſhe was hot, her eyes looked heavy, her head ached, and ſhe vomited every thing ſhe took. The Vaccine puſtules were now ſurrounded with a fiery efflorefcence as large as a half crown, which was very painful and hard. The child in Small-pox appeared much relieved by the bliſter; the reſpiration was freer, the fever and reſtleſſneſs had abated, and the puſtules had ſomewhat riſen, though they ſtill continued pale. The Vaccine puſtules were as before, and had not the ſmalleſt appearance of inflammation round their baſes; the ſame was obſervable of the two other vaccinated children, and the youngeſt looked a little heavy and ſtarted frequently.

On the tenth day the children were much the ſame; the eldeſt complained much of her head, vomited frequently, and the arms were very ſore. The puſtules on the arms of the youngeſt child but one, were this day ſurrounded with an inflamed areola, but ſhe continued well in other reſpects. In the youngeſt there was not the ſlighteſt appearance of inflammation round the puſtules; he continued ſlightly indiſpoſed and frequently ſtarted, which was attributed to dentition.

On

On the eleventh day, the youngeſt child, inoculated on the ſixth, was covered with a copious eruption of Variolous puſtules, attended with a conſiderable degree of fever, much reſtleſſneſs, and conſtant ſtarting. The Vaccine puſtules in this child exactly reſembled thoſe of the other child in Small-Pox, and had no appearance of efflorefcence round them. The eldeſt child ſtill continued very ill, complained much of her head, and vomited every thing ſhe took, which alarmed the mother leſt ſhe alſo ſhould be affected with Small-pox; but in conſequence of the areola continuing very large and beginning to aſſume a darker red, I ventured to pronounce her ſecure. After this time, the children gradually recovered their healths, and the Vaccine puſtules ſcabbed over without producing any inconvenience.

The youngeſt child during the whole courſe of the Small-pox was unuſually reſtleſs, and was affected with ſuch violent ſtartings as almoſt reſembled convulſions. Opium neither abated theſe, nor its inceſſant cries and moans; immerſion in warm water, which was obliged to be frequently repeated, procured the only relief. The child which was a ſecond time inoculated, reſiſted once more the effects of the Vaccine virus. From this it ſeems probable that her conſtitution had ſuffered that peculiar change, during the convulſions, which is neceſſary to reſiſt the Vaccine and Variolous contagion, as ſhe lived in the ſame room and often ſlept in the ſame bed with her brothers during the whole courſe of the natural Small-pox. This appears alſo to me an additional proof that the number of perſons, who from ſome peculiarity of conſtitution are ſuppoſed capable of completely reſiſting Variolous contagion, is greatly over-rated. I have inoculated a conſiderable number of perſons with recent Vaccine virus, who poſitively declared they never had had the Small-pox; but in all theſe inſtances the reſult was agreeable to what I had foretold, the punctures inflamed for three or four days and then diſappeared.

In another family, about the ſame time as the caſes abovementioned, a child was affected with confluent Small-pox, and on the 5th day of the eruption, three children of the ſame family were inoculated with fluid Vaccine matter. Two of the children were afterwards removed to another houſe, but the youngeſt, an infant not a month old, remained in the ſame room with the Small-pox patient, who had a ſevere diſeaſe, and recovered very ſlowly. In theſe three children the Vaccine puſtules went through the uſual courſe, and were ſurrounded with extenſive inflammations. One of the children on the eighth day complained of much uneaſineſs in the axillæ, and alſo of ſickneſs and head-ach; her ſkin was very hot, and ſhe ſeemed much indiſpoſed.

indiſpoſed. On the tenth day, about twenty or thirty puſtules reſembling varicellæ appeared upon the face and neck, which after continuing out four or five days, fell off. In another child a trifling eruption of hard dry ſpots appeared about the 10th day and ſoon diſappeared. Theſe are the only inſtances I have ſeen of puſtules during the Vaccine inoculation.

Though in the caſes above related, the ſucceſs of Vaccine inoculation was greater than I expected, ſince of eight children inoculated, five went through the diſeaſe in the uſual manner, whilſt ſurrounded with contagion of Small-pox, and of the two caſes of natural Small-pox, one only could have been prevented by the Variolous inoculation; yet the practice ought not to be followed, becauſe where there is a ſuſpicion of Small-pox contagion having been received, the moſt ſpeedy means ſhould be adopted to ſuperſede it, and this can only be done with certainty by the Variolous inoculation. I have inoculated children with Variolous matter after the diſeaſe had broke out in the family, with perfect ſucceſs; and in one caſe, the matter was taken from a perſon on the ſixth day of the eruption of a bad confluent Small-pox, to inoculate a child in the ſame houſe, which experienced the diſeaſe in its mildeſt form.

MEDICAL SOCIETY, LONDON, 1802.

ON Monday, March 8, the Medical Society of London, held their Anniverſary Meeting, at their houſe in Bolt Court, Fleet-ſtreet, when the Officers and Council for the current year were elected, and an Oration delivered by the Preſident, of which the following Sketch will diſcover the ſuitableneſs and importance.

He began with lamenting the occaſion of his unexpected appearance before the Society at that time, the ſudden and ſerious illneſs of Dr. Garnet, who had been previouſly appointed to deliver the Anniverſary Oration, and to whoſe ingenuity, induſtry, and urbanity, he gave juſt and highly merited commendation.

He then proceeded to the immediate object of his diſcourſe, which was to point out the various ſtudies requiſite to form the Medical Practitioner in general, but eſpecially the Phyſician, whom he had more particularly in view.

He began with preparatory ſtudies, and after the knowledge of the mother tongue, he conſidered intimate acquaintance with the Latin language as not only deſirable, but abſolutely neceſſary, both in its Claſſical and in its various inferior ſpecies of Latinity, in which latter the works of the moſt valuable ancient medical authors are written. A moderate knowledge of the Greek language was recommended, but this he ſtated to be of inferior importance, there being in general excellent verſions of the moſt eſteemed authors in that tongue. The French, both as an univerſal language, and as that of faſhion and accompliſhment, muſt by no means be neglected, to which may, perhaps, be added the German and Italian.

He then proceeded to the Sciences connected with Medicine, and after general knowledge of the Mathematics, he mentioned ſome of the branches of Natural Philoſophy, ſuch as Mechanics, Hydraulics, Pneumatics, Aſtronomy, Electricity, and above all Chemiſtry. Natural Hiſtory he conſidered not only as eſſentially neceſſary to the Student, but as equally intereſting and amuſing, particularly when not confined to the external appearances of the ſeveral ſubjects of this ſcience, but extended to the internal ſtructure and uſes of them.

Having gone through theſe preliminary ſtudies, he took a view of thoſe which more directly relate to the practice of Medicine. He began with Anatomy, a ſcience of prime importance, not only to Surgery, but alſo to the cure of internal diſeaſes; after which Phyſiology was mentioned, then Pathology, which although the moſt uſeful branch, is often moſt neglected. The extenſive advantages of the ſtudy of Materia Medicæ and Pharmacy were ſuitably noticed. In his obſervations on the methods of ſtudying the Practice of Medicine, he made ſeveral remarks on the preſent mode of conducting Clinical Lectures, which he conſidered as having departed from their original deſign of examining and deſcanting upon ſymptoms by the bed-ſide of the patient, to a mere ſyſtematic hiſtory of particular diſeaſes, and thus proportionably diminiſhed in their utility to the Student.

Having next conſidered the Practice of Phyſic as conducted by Apothecaries, Accoucheurs and Surgeons, he concluded with ſeveral very judicious reflections on the religious, moral, and prudential conduct of the Phyſician, an attention to which are unqueſtionably greatly conducive to ſucceſs and reſpectability.

The Names of the Officers and Council were then read over, as alſo were the Prize Queſtions for the three enſuing Years, after which the Society adjourned.

OFFICERS AND COUNCIL OF THE MEDICAL SOCIETY OF LONDON,

For the YEAR 1802.

PRESIDENT.

Dr. James Sims

TREASURER.

Dr. Walker

LIBRARIAN.

Mr. Hurlock

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Mr. Field
Mr. J. H. Hooper

SECRETARY FOR FOREIGN CORRESPONDENCE.

Dr. R. Hooper

COMMITTEES.

I. THEORY and PRACTICE of PHYSIC.

Sir John Macnamara Hayes, Bart.
Dr. Lettſom
Dr. Hulme
Dr. Saunders
Dr. Bradley

II. ANATOMY and PHYSIOLOGY.

Dr. Relph
Dr. Jenner
Mr. Abernethy
Mr. A. Cooper
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III. SURGERY.

Mr. Ford
Mr. Norris
Mr. Ware
Mr. Forſter
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IV. MIDWIFERY.

Dr. John Sims
Dr. Haighton
Mr. Ridout
Mr. Webb
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V. MATERIA MEDICA and PHARMACY.

Dr. Yelloly
Dr. Marcet
Dr. Curry
Mr. Jackſon
Mr. Jeaffreſon

VI. BOTANY and NATURAL HISTORY.

Dr. Bancroft
Dr. Elliot
Dr. Pearſon
Dr. Rogers
Mr. Chamberlaine

VII. NATURAL PHILOSOPHY and CHEMISTRY

Dr. Babington
Dr. Pinckard
Mr. Parkinſon
Mr. Seaton
Mr. Andree

ANNIVERSARY ORATION for 1803.

Dr. Garnett

REGISTER.

Mr. Spry

PRIZE MEDALS *to be given by the* MEDICAL SOCIETY *of London.*

THE Society resolve to give annually, a Gold Medal, called the FOTHERGILLIAN MEDAL, of Ten Guineas value, to the Author of the best Essay on a given Subject; to which the Learned of all Nations are invited as Candidates.

The Medal for the year 1803, will be adjudged to the Author of the best Dissertation on the following subject: "The "distinguishing Characters of CARCINOMATOUS TUMOURS, "and the most advantageous Mode of treating them."

Subject for the year 1804. "TUSSIS CONVULSIVA, or "HOOPING COUGH, its History and most advantageous Treat-"ment, distinguishing particularly the Effects of the most com-"mon and popular Remedies employed for its Cure."

For the year 1805. "CONTAGION; and the Means of "preventing it."

N. B. It is desired that every Answer may, as far as possible, be founded upon actual Experiments, or well authenticated Facts.

Regulations respecting the Medal.

1. Each Dissertation shall be delivered to the Secretary in the Latin, English, or French Language, on or before the first day of November, in the preceding year; and the Adjudication of the Medal shall take place in the last week of the ensuing February.

2. With each Dissertation shall be delivered a sealed packet with some motto or device on the outside; and within, the Author's name and designation; and the same motto or device shall be put upon the Dissertation, that the Society may know how to address the successful Candidate.

3. No Paper with the name of the Author affixed, can be received; and if the Author of any Paper shall discover himself to the Council, or to any Member thereof, such Paper shall be excluded from all Competition for the Medal.

4. All the Dissertations, the successful one excepted, shall be returned, if desired, with the sealed packets unopened.

*** The Society propose to give two Silver Medals annually; one of which shall be adjudged for the best Essay, read before the Society within the year, written by a FELLOW; the other for the best Essay, by a CORRESPONDING MEMBER; or by any person NOT a Member of the Society.

To

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

If you think the annexed liſt of the Diſeaſes which occurred at the Pneumatic Inſtitution during laſt month, worthy of a place in your Journal, I ſhall be very happy to continue it occaſionally.

The gentlemen who give you ſimilar liſts ſeem to differ in their Nomenclature; a greater uniformity would perhaps be deſirable, if therefore you ſhould think proper to alter that which I have followed, I ſhall be thankful for your corrections, and adopt them in future.

Many public practitioners may be prevented from ſending you monthly liſts, by the idea that an increaſe of them would occupy too much of the room, which would otherwiſe be allotted for more important information. This objection, I think, might be removed, by throwing all ſuch liſts into one monthly table, of a ſtandard arrangement, allowing a column for the numbers of each Report, and all occaſional obſervations might be collected in a maſs, after that table. Should this be impracticable, I hope you will excuſe the ſuggeſtion, as being well meant.

A detailed account of the Medical and Phyſical experiments carried on at the Pneumatic Inſtitution in the courſe of laſt year, comprehending extenſive trials of the muriate of lime in ſcrophula, and ſome other new and little tried medicines in other important chronic diſeaſes, will be publiſhed in a ſhort time. I am, &c.

J. KING.

Briſtol Hotwells,
Jan. 10, 1802.

Account of Diſeaſes in the Diſpenſary of the Pneumatic Inſtitution, Dowry Square, Briſtol Hotwells, from November 30, *to December* 31, 1801.

ACUTE DISEASES.	
Fever, Intermittent	2
——— Typhus	1
Dyſentery	1
Acute Rheumatiſm	1
Eryſipelas	1
Small-pox	1
CHRONIC DISEASES.	
Aſthma	4
Catarrh	7
Catarrh Senil.	4
Pleurodynia	2
Hæmoptœ	2
Phthiſis Pulmon.	12
Rhumatiſm. Chron.	3
Scrophulous Ophthalmia	2
Scrophulous Otitis	3
——— Glandular Swelling	8
——— Glandular Abſceſs	4
——— Affection of the Joints	4
——— Tabes Meſenterica	3
Atrophia, from Poiſon	1
Epilepſy	2
Dyſpepſia	3
——— Bilious	2
Hypochondriaſis	2
Hyſteria	3
Chloroſis	3

Dyſmenorrhœa

Dyſmenorrhœa	4
Amenorrhœa	5
Menorrhagia	1
Hæmorhois	1
Maſtodynia	1
Fluor Albus	2
Prolapſus Uteri	2
Gonorrhœa	1
Syphilis Primar.	1
——— Secundaria	3
Dyſuria	5
Enureſis	1
Anaſarca	2
Aſcites	1
Hydrocele	1
Impetigo	1
Herpes	2
Petechiæ ſine Febre	1
Contuſion	1
Ulcerated Legs	2
Vermes	3
Inoculated for the Cow-pox	2

List of Diseases at the Bath City Dispensary, from December 31, 1801, *to January* 30, 1802.

Angina	1
Abſceſs	2
Anthrax	[illegible]
Aſthma	7
Aſcites	1
Anaſarca	2
Bilious Complaints	3
Bloody Urine	2
Catarrh	14
Contuſion	1
Carcinoma	1
Chloroſis	4
Diarrhœa	5
Eryſipelas	3
Eruptiones	3
Fever	8
Gaſtrodynia	3
Gonorrhœa	1
Hæmoptyſis	2
Hæmorrhagia (inteſtinal)	1
Hæmorrhois	2
Hernia Humoralis	2
Hernia	1
Herpes	1
Icterus	2
Lepra	1
Menorrhagia	1
Ophthalmia	1
Peripneumonia Notha	2
Pulmonary Complaints	10
Phthiſis Pulmonalis	4
——— Incipiens	1
Pertuſſis	3
Phyſconia	1
Prolapſus Uteri	1
Pſora	1
Rheumatiſmus Acutus	11
Synochus Bilioſa	10
——— Pleuritica	2
——— Enteritica	1
——— Dyſenterica	1
——— Cholerica	1
Scarlatina	1
Suppuration	1
Syphilis	3
Sciatica	1
Scrophula	3
Strain	3
Typhus Gravior	4
Tertian	1
Tuſſis	7
Tumor Mammæ	1
Urticaria	1
Ulcerated Legs	7
Variola	4
Total	161

Bath, Feb. 7, 1802.

W. WHITE.

ACCOUNT

ACCOUNT OF DISEASES IN AN EASTERN DISTRICT OF LONDON,

From February 20, *to March* 20, 1802.

ACUTE DISEASES.	
Typhus Mitior	2
Pneumonia	4
Peripneumonia Notha	10
Dysenteria	5
Rheumatismus Acutus	3
CHRONIC DISEASES.	
Catarrh	4
Tussis	33
Dyspnœa	10
Tussis cum Dyspnœa	12
Hydrothorax	4
Pleurodyne	5
Palpitatio	2
Apoplexia	1
Paralysis	2
Vertigo	3
Cephalalgia	6
Vomitus	2
Dyspepsia	7
Enterodynia	2
Colic	1
Constipatio	3
Dysuria	4
Hæmorrhois	2
Herpes	3
Lumbago	3
Rheumaticus Chronicus	21
PUERPERAL DISEASES.	
Ephemera	2
Menorrhagia Lochialis	4
Mastodynia	3
Prolapsus Vaginæ	1
INFANTILE DISEASES.	
Rubeola	5
Pertussis	6
Aphthæ	3
Ophthalmia	2

The observations respecting the rapid changes of the weather, which were made in the last Report, may with propriety be repeated in the present. During the last few weeks, the wind has blown from very different points, and the temperature of the air has been much varied. The same species of disease, therefore, as might well be expected, still prevail. The attention of medical practitioners has of late been very much engaged by the diseases of children: A great number of this class of patients have been visited by the measles, the symptoms of which have, in some instances, appeared in their most threatening form. In the mildest form of this disease, the fever generally abates after the eruption has appeared; but in the cases referred to, the fever continued, and was accompanied with such symptoms of pneumonic inflammation as to excite considerable alarm, and, in some instances, proved fatal. When we remember that a mild disease sometimes succeeds a considerable degree of what has been called the eruptive fever, our prognosis respecting the termination of the disease cannot with any propriety be formed previously to the eruption, and must then be determined, principally, by the degree to which the thoracic affection prevails. It has been a question whether blood-letting, which is a powerful means of reducing inflammation, should be had recourse to in the early part of the disease or to-

wards

wards the cloſe of it. This ſhould be determined rather by the degree of inflammation than by that of eruptive fever, ſince, as it has been obſerved, a conſiderable degree of fever often precedes a mild diſeaſe, in which the cough and other pneumonic ſymptoms do not wear a threatening aſpect. The blood-letting therefore has in ſome caſes, with great propriety, been deferred to a later period of the diſeaſe, even till after the desquamation has taken place, at which time the fever and cough, and other ſymptoms of thoracic inflammation, often appear in their moſt alarming form, and ſoon put a fatal period to life; or elſe lay a foundation for a more permanent affection of the lungs, terminating in the equally fatal phthiſis pulmonalis.

State of the Patients admitted at the Bristol Dispensary, from the 1ſt of December, 1801, *to the 1ſt of January,* 1802.

Aſthma	3	Menorrhagia	1
Anaſarca	1	Nervous	1
Aſcites	1	Pleuritis	5
Cynanche Tonſillaris	2	Phthiſis Pulmonalis	3
——— Maligna	1	Peripneumonia	6
Conſumption	4	Pertuſſis	3
Dyſenteria	5	Rheumatiſmus Acutus	1
Diarrhœa	1	Scrophula	3
Dyſpepſia	3	Scorbutic	1
Eryſipelas	2	Synocha	14
Febris Puerperalis	1	Typhus	16
Gaſtrodynia	2	——— Putrida	1
Gravel	1	Tuſſis	2
Hydrothorax	1	Tinea	1
Iſchuria	2	Variola Putrida	1
Lumbago	1		

Patients admitted in the courſe of laſt year		1353
Cured	1108	
Relieved	45	
Died (many of whom where in the laſt ſtage of conſumption)	103	
Remain ſick	97	
Total of ſick patients in twenty-ſix years		15135
Midwifery patients delivered at their own houſes this year		309
Children born { Boys 166, Girls 148 }	314	
Remain undelivered		67
Total of midwifery patients in twenty-four years		4884
Children born { Boys 2545, Girls 2379 }		
Total, in 24 years,	4924	

Dispensary House, Jan. 9, 1802.

W. D. ROLFE.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IF the enclosed be deemed worthy of insertion in your very valuable Journal, I beg leave to offer it for the same.

Your's, &c.

Victualing Office Square, Tower Hill.

W. WARNER,
Member of the Royal College of Surgeons.

MUCH has been said in your esteemed Journal on the beneficial effects derived from the external application of Opium in several complaints of the human body; and its use has been strikingly beneficial in relieving patients from the excruciating pain of excessive cramp in the bowels and lower extremities, attendant on an alarming disease, (when neglected by patients, and suffered to continue without calling in medical assistance) viz. Cholera Morbus; and when Opium has been ejected immediately on being taken into the stomach, as I have several times witnessed.

Of all the periods of life, none seem to demand our particular attention to relieve the many afflictions thereof so much as that of Childhood. Enough cannot be said in praise of the discovery made of the Cow-pox, when we consider the dreadful malady, with all its fatal symptoms, which most frequently attack the above period, and which will be now thoroughly overcome.

Another disease most incident to the aforementioned period, and very distressing to the sufferer as well as tedious in its duration, by which very many are destroyed, is the disease termed Hooping Cough.

Having been the subject of this disease at an adult age, my thoughts were often engaged with a strong desire to relieve (I will not say cure) in future cases speedily. Nothing seemed so likely as the use of Opium; but it being dangerous to be put into the hands of patients, and opportunity not offering for its use, it being a common received opinion that no remedy will succeed but change of air, I almost relinquished the idea, till hearing that four patients, whom I knew had it severely, were relieved by the use of opium alone. (It was recommended by a person not of the profession.)

An opportunity now offering in my own family, an adult subject, at my request he made use of the remedy; at the same time

time it was applied to a patient between two and three years of age. Both subjects being very different in all circumstances, I will take the liberty of relating both cases.

S. B. 21 years of age, a healthy constitution and sanguine temperament, was attacked with cough, supposed from taking cold, which increased in violence about three weeks, during which time a variety of balsamic medicines, with anodynes, were taken, but to no good effect. Soon, from its violence, it became alarming; the fits of coughing were attended with hæmorrhage from the nose, and terminated either with vomiting, or a state almost amounting to suffocation; which left no doubt of its being hooping cough: A violent soreness of the abdominal muscles was occasioned by its urgency; and debility, to a great degree, was experienced.

Fourteen ounces of blood were drawn from the arm, which shewed no signs of inflammatory affection, but in every respect in a healthy state. Liquid laudanum was rubbed all over the abdomen and pit of the stomach, twice a day, (without taking any medicine) which gave him great relief; the cough abated in frequency and violence; vomiting and hæmorrhage both ceased. This was continued for about a fortnight, when the cough almost totally subsided, and his usual state of health returned.

The next case was a subject very unpromising for so violent a disease, and in whom opium was also serviceable.

I. P. between two and three years of age, a weakly habit, much inclined to rickets and scrofula, was attacked with hooping cough; and when I first saw him, very unfavourable symptoms were present; the cough almost incessant, breathing very difficult, countenance swollen and rather livid, very feverish, and refusal of all food; no sleep, but comatose. The laudanum was used as in the former case; and, in addition, small doses of the decoction of the yellow bark were given three times a day, and the bowels now and then relieved by a gentle purgative. Good effects were visible; in a few days the above symptoms went off, and the appetite returned. The bark was left off, and the laudanum used for about a fortnight longer, when the child was recovered.

Another, in the same family, only eight months old, is now under the use of the same remedy, with only a laxative now and then, and is recovering.*

Dr. Cullen in his excellent work, (vol. 3,) speaking of this disease says, "When the contagion is recent and continues to act

* Nine or ten cases have succeeded with using this remedy.

act, we neither know how to correct or how to expel it; and therefore the disease necessarily continues for some time: but it is probable that the contagion in this, as in other instances, ceases at length to act; and that then the disease continues, as in other convulsive affections, by the power of habit alone."

After directing various means, according to different stages of the complaint, he mentions the several antispasmodics; he relies most on opium, but there I presume is meant the internal use; whereas, we know the uncertainty of the dose, as well as the inconvenient effects produced by it in children as well as in adults. On the other hand, its external use promises the greatest benefit.

Many remedies are used for this disease, but the one most used, is an outward application, called Roche's Embrocation, which, in three instances, ill effects appeared to arise from the cough suddenly stopping after the use of it; two died in strong convulsions, the third was relieved from approaching suffocation by a smart purge and an emetic, and is now living.

The laudanum moderates each paroxysm in frequency, &c. therefore, if early used, prevents the expectoration which is otherwise caused, and though the cough goes on some time, it evidently shortens the usual paroxysm. In order to disguise the laudanum, a few drops of æther may be added, and in this way used as an embrocation twice or thrice a day, over the stomach and belly.

A flannel waistcoat should be worn by the patient, as, no doubt, it promotes absorption, and prevents the vicissitudes of the climate taking that effect on the skin, which we know it does, and acts as an exciting cause of coughing.

Perhaps I am premature in offering this to the public notice; but the disease being so very prevalent at present, and consequences so generally fatal, I thought it might excite others in the profession to make trial of the same remedy; and if with the same success, I hope it will plead a sufficient apology.

A successful Mode of preventing Gangrene in compound Fractures;

As practised by Messrs. William, Robert, and John Crowther, *in the West Riding of Yorkshire.*

Communicated by Mr. John Crowther, *resident at Halifax.*

It may be proper to premise this account of the manner of treating compound fractures, by observing, that the method now

now recommended has been practised, with the greatest success, by our ancestors, in Eland, near Halifax, time immemorial, being handed down from father to son.

When we are called to a patient who has suffered a compound fracture, (of the leg for example) we first endeavour, as expeditiously as possible, to clear the wound of all extraneous bodies, and then put the limb by a gentle extension into a good position, the knee being kept at the same time moderately bent, to favour the relaxation of the muscles of the fractured limb. While this is performing, we order a large quantity of black basilicon ointment, made with tar instead of pitch, to be liquefied in an iron or earthen pan, and made hot, then put into it with all dispatch a large pledget of tow, sufficient if possible to cover the whole wound, so as totally to exclude all external air. If one pledget be not sufficient to cover all the lacerated parts, then another must be applied. Our manner is generally to spread out the tow, take hold of each end, dip the middle part into the hot ointment, then lift it out, and apply it directly to the wounded part. The leg being raised and kept in a proper position, as said above, a roller about two inches broad is applied in such a manner as to give liberty to the future swelling of the limb. The roller, before its application, is always moistened in alegar or vinegar (we generally use the former), with a solution of saccharum saturni in it, in the proportion of about half an ounce to a pint of the liquid. We begin at the foot, and roll upwards as high as, or above, the knee, rather tight at the ankle, if the fracture be not there, but always gently wherever the fracture is, taking care at the same time that the great toe is in a line with the patella, and the sole of the foot nearly square with the heel; the latter rule is intended to prevent a retraction of the heel, which would hinder the patient after the cure from stepping fairly and even upon the ground, when he at first attempts to walk. Upon this roller we apply pasteboard, cut broad at the top and narrow at the bottom, of the length of the leg, and three or four in number, as occasion may require. These are to be soaked well in warm water, and fastened close to the limb by another roller (not applied very tight) of the same breadth as the former, and moistened in the like manner. The pasteboard is moistened that it may, when dry, take the shape of the limb, and be more easily and expeditiously applied at the next dressing. Then come the splints, one of which is to be large, and always put underneath the leg, so as to reach down from the bend of the knee to below the heel, to keep the inferior fractured parts from sinking, and thereby prevent a prominence of the superior parts of the bones. The splints are always lined with soft linen, tow, &c. to fill up

up all the cavities of the limb, and lie eafy; and this lining is faftened to the fplints, that they may always come off together, and thus expedite the dreffing. And particular care is always taken that a large foft compreſs or pad be placed underneath the heel, left it fhould heat and inflame, and become intolerably painful. And, laftly, we place the limb upon a pillow, and tie it up with three or four pieces of tape at equal diftances, faftened with bow-knots, to be the more eafily loofened. If the patient be tolerably eafy, we do not take off the dreffings for one, two, or fometimes three days; but if otherwife, we infpect the wound the following day, to examine the caufe and remove it. The fecond dreffing is made in the fame manner as the firft, with this difference, that the ointment is not applied quite fo hot, nor the bandage fo loofe as before, unlefs inflammation, or other circumftance, fhould indicate the propriety of a contrary treatment. At the third dreffing, the ointment is applied ftill cooler, and fo on; but always continued as a warm dreffing, until there be a free and laudable purulent difcharge; which being once accomplifhed, the ointment is ufed quite cool to the end of the cure, unlefs fome contra-indication fhould arife, fuch as proud flefh, &c. The fame moift bandages, and their manner of application, are generally continued until the inflammation of the limb abates, and then they are ufed in a dry ftate. In about a week or ten days we commonly begin to be more circumfpect at every dreffing in fhaping the limb, by making greater extenfions, bringing the fractured parts of the bones more into contact, clofing the lips of the wound, &c. for, before this time, we are under no great apprehenfion of any little irregularity in the coaptation of the ends of the bones. Indeed, for fome time after this period, we compare the callus to foft glue, which we can fhape and mould nearly as we pleafe; and for this reafon we do not difturb the fracture too much by extenfions, &c. at the firft, while the parts are in an inflamed and irritable ftate. The perfect formation of the callus, and union of the bones of the fractured leg of an adult, of a middle age, we generally find to take place in about twenty-five days, provided there is no lofs of bone. And what is remarkable, we have fometimes known, in fimple fractures, the callus to ftiffen and unite very fuddenly; for inftance, the limb has been fomewhat flexible at the fractured part the preceding day, and ftiff the next.

We always examine the pulfe at every vifit, and form a particular judgment by it; if it be full and ftrong, and not very quick, as from 70 to 90 in a minute, we augur well of the patient; but if it be quick and weak we efteem it a bad fign. In the beginning we prefer coftivenefs to an open body, and

and wish it to remain so for the first week or ten days, as we think that it keeps up the patient's strength, and produces a vigorous circulation in the vessels and lacerated parts, which it is our endeavour to promote; and has also this further advantage, that the limb is in no danger, in the early stage of the cure, of being disturbed and shaken by the preparation necessary for an alvine discharge. In compound fractures there is usually a great loss of blood; but if this should not be the case, and there come on much swelling and inflammation, with a full strong pulse, (which in our practice rarely happens) we do not hesitate to bleed freely, and give saline cathartics, such as sal. Glauber, &c. and, if necessary, apply leeches to the tumefied parts, and fomentations, and cataplasms of oatmeal and grounds of beer boiled together, but always the ointment before mentioned upon the wounded parts. We recommend it to our patients to sit up in bed every day, conceiving it to be a great means of both promoting digestion and preserving the strength, and far preferable to their being confined in a supine posture for such a length of time. The patients are generally kept to their usual diet, but we forbid beer, wine, and all spirituous liquors for drink, unless they have been much accustomed to any of them, and then allow a moderate quantity. For common beverage we prefer tea, barley-water, and milk. But if the wounded person be of a cold phlegmatic constitution, and the pulse weak, we give Peruvian bark liberally, and allow a more generous diet and some wine, with the intention of strengthening the habit, and keeping up the circulation in the wounded and lacerated parts; for we fear nothing more than a pale or sallow complexion, a weak quick pulse, and defect of a glowing febrile warmth.

As we live in a part of the country which abounds with coal mines and stone quarries, and is full of manufacturers, many of whom are employed in the direction of dangerous machines worked by steam-engines, we are frequently called upon to attend very serious accidents. From the 28th of April, 1798, to the 5th of October, 1800, not fewer than ninety-eight fractures have fallen to my share, twenty-eight of which were fractures complicated with a wound; but which were never followed by any gangrenous appearances. The same success has attended the practice of my two brothers, William and Robert, for many years.

Abstract of a Communication from a Physician in Philadelphia, dated December the 10th, 1801, to Dr. Patterson, *in Londonderry.*

Dear Sir,

Having seen two vessels advertised for Londonderry, I eagerly embrace the opportunity to acknowledge the receipt of your letter, under date of the 25th of last April, and to congratulate you on the return of peace to your country as well as to the rest of Europe.

I am very much obliged to you for the extracts you favoured me with from the book, entitled 'A General History of the Air,' dedicated to Dr. Mead, relative to copious venesection in pestilential fevers. Whether Mr. Webster's Collection on the Precursors and Causes of Pestilential Diseases has been anticipated by that author, as you suspect, I cannot say; but from the primitive absurdity of the opinions of Mr. W. I cannot suppose it has been the case.*

The Communicator proceeds to mention Dr. Haygarth's letter, addressed to the College of Physicians in Philadelphia, and cites some passages from it, which need not be transcribed here, as Dr. Haygarth's tract is before the public. He particularly regrets, with Dr. Haygarth, the bad consequences which resulted from the cause of the pestilence in 1793, being ascribed to '*Putrid coffee*' by a physician of eminent abilities, and he ridicules the notion of Mr. Webster, who imputes the rise

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* The passage above alluded to is given on the authority of 'Clark's Examples,' where, noticing the London plague of 1665, which was imported from Holland, December, 1664, in some bales of cotton, it is remarked, 'Of this plague died within the Bills of Mortality near 100,000. O fatal bales of cotton! Yet the true remedy was never discovered, viz. Bleeding *ad deliquium animi*, at the first instant of seizure. But the reason of this is yet a secret.' This expedient of profuse blood-letting I have shewn in my '*Remarks on Dr. Rush*,' published in 1795, was an ancient practice in fevers, under certain provisional rules. In Britain and France, anno 1545, raged a pestilential epidemic, called *troup galant*, because it chiefly seized and carried off young, strong, brisk people, in which *large* and *repeated* blood-letting was said to be the only help; and I find, that, in the pestilence which prevailed in Milan, Padua, &c. in 1575, a similar evacuation was practised by Palmarius. At the same time, I observe, that, in the *generality* of inirritative epidemic fevers, described in ancient records, venesection, in every stage and degree, was found to be pernicious. W. P.

rise of the pestilential epidemics to volcanos, earthquakes, halos, high tides, hail-stones, dead haddocks, &c. in times and countries distant from the juncture and place in which those diseases made their appearance. †

You mention, continues he, having sent a Reply to the Animadversions of the Editors of the New York Medical Repository, in expectation that you should have an opportunity of defending yourself. No such reply has yet appeared in the Repository that I have seen; and if it in any degree controverts the hypothetical doctrine of one of the Editors, I question much whether it ever will appear: For it is their practice to reject every thing unfavourable to a cause, which receives its chief support from the art of rendering all the avenues to truth impervious and remote, and the plainest facts obscure and intricate. This has been strongly exemplified with regard to the account, which they lately published, of the origin of the pestilential fever at Cadiz, in the hot season of 1800. In this account, they affirm that the disease originated in Cadiz from local causes, and had no connexion with imported contagion.

In consequence of this glaring attempt to mislead, the following paragraph appeared in the New York Gazette of the 28th of October last. 'The Conductors of the Medical Repository have given another specimen of their great partiality for such opinions and supposed facts, as in the remotest degree countenance their doctrine of the domestic origin of the pestilential fever, which of late years has occasioned such dreadful mortality in this country; while with studied caution they reject every thing that militates against it, although derived from the most authentic sources.

'In the last Number of the Repository for the present year, they have published a mutilated abstract of a Supplement to the Madrid Gazette, dated the 28th of October, 1800, relative to the

† In the 'Remarks' referred to in the preceding note, I have given reasons and instances to prove, that neither the effluvia from vegetable nor animal putrefaction can generate a specific pestilential disease; and from subsequent inquiries, I am furnished with additional arguments to confirm me in this opinion. The gentlemen, who resolutely made experiments on the matter of *black vomit*, in the yellow fever of America, received no injury; and, in the general plague of 1347-8, 'Tanners, curriers, such as cleansed bog-houses, servants in hospitals, and those employed in other nasty stinking businesses, all escaped infection; wherefore I am led to believe, that the notion of '*Putrid coffee*,' &c. wants, and ever will want, the *consent of time*. As to the fanciful causes alledged by Webster, we shall only condescend to ask, Do volcanos, showers of blood, swarms of mice or of grasshoppers, enchanted coffins, red or green crosses, handling a dead priest's bones roughly, all produce similar effects on the atmosphere, and beget the same pestilential distempers? W. P.

the origin and treatment of the fever at Cadiz; which, as it contains nothing decisive on the subject of its introduction, but specifies merely the state of the weather previous to its occurrence, and during its prevalence, these gentlemen have *modestly* construed into a proof of its having originated there from local causes. But they have taken special care to pass by the facts of a later date and different complexion, published in the same paper, of the 4th of November, 1800, entitled, "Some particulars relative to the sickness which has depopulated Andelusia;" the contents of which follow.

'At Cadiz it blew a strong easterly wind, which, passing over a burning part of the country, augmented the excessive heat of the summer, whereby the atmosphere was well disposed to receive the sickness, but by no means was the *cause* of it. The greatest part of the physicians of Cadiz were wrongfully impressed with this opinion, and therefore applied wrong remedies, which increased the mortality. On the 8th of August, an American vessel entered the Harbour of Cadiz; the log-book of the captain mentions, that during the passage three men died on board of the *yellow fever*. The crew, coming on shore, went into the neighbouring streets and taverns, and all died of the sickness excepting the mate. The fever soon after shewed itself among the inhabitants of the city, and there was not a house into which the infection did not penetrate. Terror spread on all sides. Many of the inhabitants, not knowing that they already had the sickness in their bodies, fled to the Isle De Leon, and seven other places, to the inhabitants of which these, perhaps, fugitive emigrants soon communicated the disorder, which was confined to the nine cities, and did not extend to Estramadura. From the 12th of August until the 1st of November, died of this sickness seventy-nine thousand five hundred persons.'*

It did not appear in any of the sea ports of the neighbouring nations in the same climate; of course, the condition of the atmosphere could have had no share in its generation. I have also been assured by Mr. Patrick Yznardi, son to the American Consul at Cadiz, who lost several of his connexions by the disease, that it was introduced into Cadiz by a vessel from Havanna, and that the Intendant of Havanna, who was passenger on board that vessel, had been imprisoned by order of the ma-

* In the places taken generally, the proportion of deaths was from a fourth to a fifth part of the number of inhabitants; and in Seville the mortality was so great, that three-eighths of the people fell victims to the pestilence. W. P.

giſtrates for neglecting to inform the Commiſſioners of Health of the exiſtence of the diſorder in the ſhip; likewiſe, that ſome of the phyſicians were ſeverely cenſured and reprimanded for miſleading the magiſtrates with reſpect to the contagious nature of the diſorder. This has alſo been confirmed by the late Spaniſh Conſul, Don Joſeph De Viar.

All the commercial towns in this country (America) eſcaped the peſtilential fever laſt year, (1800) except Norfolk, Baltimore, and Providence; the former about 400 miles ſouth, and the latter 291 miles north eaſt of Philadelphia. In the preſent year they all have eſcaped, except Norfolk and New York; and the latter was affected only partially, owing to the advanced period of the warm ſeaſon when it occurred. Theſe circumſtances, with the circumſtances attending its preſence in former years, and the fact admitted by *all parties*, that it never has been obſerved to make its firſt appearance in the moſt marſhy and unwholeſome ſituations, remote from the ſea port towns, even in the ſouthern parts of this country, where, it is acknowledged by every one, that the remote or efficient cauſe of bilious or remitting fevers is moſt abundant, are ſatisfactory proofs that it is not derived from the ſame cauſe, and baffle all pretenſions to explanation on the principle of an epidemic or peſtilential conſtitution of the atmoſphere.

It muſt appear ſurpriſing to you, that diſputes, with regard to the moſt efficacious methods of treating the peſtilential fever, ſhould ſtill exiſt among the phyſicians of this country, as well as among the Medical Gentlemen in the Weſt Indies. This, I believe, can only be accounted for, from the unfortunate propenſity in phyſicians to theoriſe, rather than to obſerve. The fondneſs for a favourite diſcovery, I ſuſpect, has miſ-led the judgment of Dr. Chisholm, with reſpect to the power of Mercury; and the diſſatisfied mind of Dr. Jackſon has led him to change his opinion relative to the nature and cauſe of this deſtructive diſeaſe. His pages teem with the ſpirit of diſcontent and oppoſition. Dr. Chisholm has detailed many of his facts in ſuch a looſe, incoherent, and ſlovenly manner, and has viewed objects ſo much through the vague and unſettled medium of *theory*, in his new edition, that he has greatly diminiſhed the weight of his authority.

The employment of mercury in ſmall and repeated doſes, for the purpoſe of ſpeedily exciting ſalivation, is the faſhion of the day in this country, as well as in the Weſt India Iſlands, not only in the peſtilential yellow fever, but in almoſt all other diſeaſes, even in thoſe of an oppoſite nature. This new and incongruous practice is defended on the principle taught by the late John Hunter, that two actions of different force cannot exiſt

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in the human ſyſtem at the ſame time. By introducing a ſtronger power, therefore, into the ſyſtem, they expect to diſarm the weaker, and thereby to ſubdue the diſeaſe, which they conſider to be only an aſſemblage of diſordered action; conſequently, they direct all their views to create an artificial diſeaſe in the mouth and fauces as ſpeedily as poſſible, that the ſtronger action thus produced may overcome the weaker. But if any dependance could be placed on this principle, pain and inflammation excited by other means would have the ſame effect, and every variety of fever might be cured by covering the whole body with bliſtering plaſters, or wetting it with cauſtic fluids, and giving ardent ſpirits and ſpices internally; which, according to the principles taught by Brown, would wear down the excitability to the healthy point.

Relying on the validity of this principle, ſome have even employed mercury in the cure of phthiſis pulmonalis, in oppoſition to the dictates of common ſenſe, and the experience of ages.

In the few caſes of acute rheumatiſm, in which I have ſeen a ſalivation induced, it has invariably aggravated the fever, and rendered the pain intolerable; and there is reaſon to believe, that mercury has a ſimilar effect in all other inflammatory affections in their acute ſtate, excepting when it operates freely as a purgative.*

I have ſeen numerous inſtances of patients with intermittent fever who have been ſalivated, without any ſenſible effect being produced on the fever. During the exacerbation of the diſeaſe, the flow of ſaliva was generally ſuſpended, but it flowed again during the intermiſſion. A ſimilar fact was obſerved by Granger during the campaign in Flanders, in 1746 or 7. This affords a ſtrong argument to prove, that ſalivation does not occaſion a ceſſation of the febrile ſymptoms of peſtilential fevers; but, on the contrary, that the flow of ſaliva is owing to the ceſſation, or ſolution of the fever.

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* Theſe ſtrictures of my worthy Correſpondent, whoſe practical authority I hold in high eſtimation, muſt be received with conſiderable limitation in *this country*, as, if fully admitted, they would tend to deter phyſicians from the uſe of mercury, in any form or degree, in caſes of acute rheumatiſm; whereas, my experience leads me to approve of its exhibition, not only in the inflammatory ſtate of this diſeaſe, but in pleuritic caſes even whilſt the ſtitch is ſharp; and at the moment of writing this note, I have under treatment a caſe of the former malady, wherein ſmall and repeated doſes of mercury, combined with opium and antimonial tartar, have produced a manifeſt good effect. For ſome time paſt, indeed, practitioners in theſe iſlands have found mercury, thus aſſociated, to be ſignally curative in inflammatory affections. W. P.

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The action of mercury on the salivary glands never removes the eruptive fever of Small-pox, nor diminishes the number of pustules. In a recent case of Measles, salivation had no effect in preventing the eruption; but it certainly augmented the fever, and added to the distress of the cough as well as of other inflammatory symptoms, by occasioning a painful inflammation of the gums and tonsils.

The Measles, which were imported to New-Castle and Wilmington on the Delaware, in the summer of the present year, by some vessels from Ireland, on board of which a great many children had died during the voyage, soon after their appearance in those places, broke out in this city, and were first observed in a school at the southern extremity of the town, kept by Mrs. Watson, where upwards of twenty of the scholars were attacked within two or three days of each other, though it was not known that any person had come into the school with the disease, or who had lately recovered from it. These scholars, residing in different parts of the town, communicated the disease to others, and so on in succession, till it has now become epidemic or general throughout the city. This is contrary to the observations of Sydenham, who confines its rise and progress to the vernal season; and says, it declines after the summer solstice as rapidly as it increases previous to that period.

If the masters of the vessels from Ireland had followed the example of many of those from the West Indies, and had denied the existence of the Measles on board their ships, physicians, who are more accustomed to speculate than to observe, would doubtless have ascribed the appearance of the disease, in this city this year, to an epidemic morbillous constitution of the atmosphere, and probably would have imputed the absence of the pestilential Yellow Fever to the superior force of the Measles in occupying the air, and banishing all other distempers from the sphere of its dominion, or compelling them to do homage to it, by wearing its livery. But the facts on this occasion are too clear to give any countenance to such a subterfuge. If the pestilential constitution prevailed, the Measles, in the majority of cases, should have been accompanied with pestilential symptoms, but it has not been so; on the contrary, in the majority of cases, the symptoms have been very mild.

In consequence of the respectable testimony in favour of the superior advantages to be derived from substituting the Kine-pox, for the purpose of banishing the Small-pox from the catalogue of diseases, Dr. Waterhouse introduced it last year into Cambridge, near Boston; and, in a considerable number of persons whom he inoculated with the matter of Kine-pox, and afterward

afterward with the virus of Small-pox, it has ſupported the character which it has acquired in the united kingdom of Great Britain and Ireland.

The generality of the phyſicians in this city alſo propoſe to ſubſtitute it in the enſuing ſpring. It has already been introduced here, and is making progreſs; ſo that we ſhall be able to procure a ſufficiency of matter for the purpoſe. I ſhould have inoculated with the matter of Kine-pox two years ago, having received an infected thread from Dr. Pearſon; but I was deterred at that time, from the fact mentioned by Dr. Jenner, of a perſon being liable to be infected with the Kine-pox more than once, though it rendered him for ever ſecure againſt taking the Small-pox. To ſubſtitute the Kine-pox, therefore, for the Small-pox on theſe conditions, appeared to me too much like exchanging a temporary evil limited in extent, for one of frequent occurrence, and of which we ſhould be in perpetual danger.* But the proofs of the ſuperior mildneſs of the Kine-pox, and the great difficulty and extreme little chance of ever taking it in the natural way, lately collected and publiſhed by Jenner, Ring, Lettſom and Aikin, together with the candid and ſummary ſtatement of Dr. Pearſon in Duncan's Annals of Medicine, for the year 1800, have removed all my apprehenſions, and of courſe all my objections to it.

Mr. Lee, a medical ſtudent in the Pennſylvania Hoſpital, has attempted (and in my opinion with ſucceſs) to refute the hypotheſis of Dr. Mitchill reſpecting the gaſeous oxyd of ſepton, or in plain Engliſh, the nitric acid, in a volatile or gaſeous ſtate, by the following facts and arguments.

'Nitrous air never exiſts in the atmoſphere, but is inſtantly converted into nitrous acid. The nitrous acid moreover has never been found in the atmoſphere in a free ſtate, but is always the product of putrefaction; and as it is generated, it unites with other bodies, particularly with ammonia. This truth Dr. Mitchill himſelf acknowledges, although he ſtill believes that nitrous acid is the cauſe of peſtilential diſeaſes. But if it ſhould be conceded that the nitrous acid is ſometimes found perfectly free in the atmoſphere, it cannot retain its freedom long enough, eſpecially in cities, to act as a cauſe of general complaint, becauſe it would very ſoon combine with other bodies. The ſubſtance for which it has the greateſt affinity is the vegetable alkali, next to this the foſſil alkali, then barytes, and afterward

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* All grounds for ſuch apprehenſion are removed by knowing, that the Vaccine diſeaſe is *not infectious* or communicable through the medium of the air and reſpiration, but that its virus muſt be *inoculated*, in order to impart it to the human ſubject. W. P.

afterward lime or calcareous earth. As the firſt three are not very abundant in cities, lime would be the ſubſtance with which it would form the chief combination.

'Calcareous earth is very plentiful in the cities of the United States, as it forms a very conſiderable part of the buildings. If therefore the gaſeous oxyd of ſepton* exiſted in the atmoſphere of cities, a union would neceſſarily take place between it and the lime; but that no ſuch union takes place is evident, becauſe every chemiſt knows that the ſalt formed by theſe two ſubſtances (nitrate of lime or nitrous ſelinite) is readily ſoluble in water, and very deliqueſcent. If the fact was otherwiſe, the houſes would ſoon fall to pieces. It is clear, therefore, from theſe conſiderations, that the nitrous acid does not exiſt in the atmoſphere, and that it cannot be the cauſe of peſtilential or other epidemic fevers.

'If the acid of putrefaction, or nitrous acid gas, was the cauſe of peſtilence, how comes it that chemiſts breathe nitrous vapours in their laboratories for hours together with impunity? And that phyſicians adminiſter to their patients large quantities of nitrous acid without the leaſt injury?

'The nitrous acid is never converted into a permanent gaſeous ſtate by the heat of the ſun in the hotteſt climate on earth; neither is the muriatic acid. To give theſe two acids aeriform elaſticity, a conſiderable degree of heat, together with the agency of a third body, is neceſſary.'†

That no ſuch gas as that called by Dr. Mitchill, Gaſeous Oxyd of Septon, exiſts in, or can be derived from, the perſpirable matter, is fully proved by the judicious experiments and obſervations of Mr. Abernethy. For theſe experiments demonſtrate, that the perſpirable matter conſiſts entirely of Carbonic gas and Azotic gas, in the proportion of a little more than

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* "This is preciſely the ſame ſubſtance as Carm. Smyth's nitrous vapour, employed in the naval and military hoſpitals with ſuch ſucceſs in deſtroying the cauſe of fever." American note.

† They who are in danger of being entangled in the perplexities which involve the hypotheſis of *Septon*, (azote or nitrogene,) or ſimilar ſpeculations, will find an excellent admonition contained in the following plain and elegant language of the preſent Prof. Gregory: "Antiquum enim et uſitatiſſimum apud medicos eſt, ſibi aliiſque perſuadere ſe rem intelligere, cum nomen tantum didicerint, novamque rem ſe credere inveniſſe, cum novum tantum rei antea bene notæ nomen impoſuerint. Caveant autem medici, caveant imprimis medicinæ ſtudio incumbentes, ne hoc modo ſibimet ipſis dent verba, umbram que pro ſubſtantia captent.") (*Conſpectus, &c. Auditoribus ſuis, p. 7.*) However, Mr. Davy having found, that gaſeous oxyd of azote is reſpirable when perfectly free from nitrous gas, and appears to ſupport life longer than common air, the ſeptic theory of contagion, as this ingenious chemiſt obſerves, is of courſe completely overturned. W. P.

than two-thirds of the former to ſomething leſs than one-third of the latter.†

Dr. Chiſholm has alſo combated the doctrine, from the circumſtance that putrefiable materials of marſhes do not afford a ſufficient quantity of azote for the formation of nitrous acid, either in a fixed or volatile ſtate.

Dr. Ramſay, of Charlestown, has publiſhed an Oration, which he delivered before the Medical Society of that city, the 1ſt of January laſt, "On the Improvements in Medicine in the Courſe of the Eighteenth Century," wherein he has acquitted himſelf very handſomely, though the limits of the Oration have compelled him to treat the ſubject too ſuperficially to make it very intereſting, except as a memorandum of names and dates.

Dr. Barnwell, a native of Ireland ſettled in this city, whoſe opportunities of making obſervations in different climates have been conſiderable, and who appears to be a man of erudition, has now in the preſs a volume, entitled "Sketches of Medical Geography, &c."

A Compendium of Chemiſtry, by J. Parkinſon, of London, with an Appendix, containing an Account of the principal Objections to the Antiphlogiſtic Syſtem of Chemiſtry, by Dr. Woodhouſe, Profeſſor of Chemiſtry in the Univerſity of Pennſylvania, is alſo printing in this city.

The ſecond Number of the fifth Volume of the New York Repoſitory, due laſt month, has not yet made its appearance; owing, I believe, to Dr. Mitchill, one of the Editors, being elected a Member of Congreſs, and being under the neceſſity of attending the ſeſſion (which takes place this month) at the Federal City.

Thoſe ſingular inſects, called *Cicadæ Americanæ*, or locuſts, which make their appearance in this country regularly every ſeventeen years, and come from a great depth beneath the ſurface of the ground, paid us their periodical viſit laſt year in numerous ſwarms; but, from the luxuriance of vegetation, they did no material damage, except to the tender foliage of the foreſt and a few fruit trees.

Notwithſtanding

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† The conſtituent principles of the cutaneous perſpirable matter, according to Plenck, are water, animal gas or carbonated hydrogene, and azotic gas. Sweat, according to the ſame author, conſiſts of cutaneous perſpirable matter, glandular ſmegma, and the ſerum of the blood. The ſmegma is an oily, ſebaceous ſubſtance; and the ſerum of the blood contains water, albuminous gluten, jelly, aerated ſoda, and culinary ſalt. The property of changing blue vegetable juices red, which is aſcribed chiefly to the ſweat of gouty people, is ſaid to proceed from phoſphoric acid. W. P.

Notwithſtanding the great mortality which Philadelphia and New York have experienced of late years, in conſequence of the peſtilential yellow fever, the population has increaſed amazingly in both. In the year 1791, the number of inhabitants in Philadelphia, including the northern and ſouthern liberties, did not exceed 52,000; this year, according to the late cenſus, they exceed 72,000. New York has increaſed ſtill more rapidly; the number of its inhabitants, at preſent, exceeding 50,000, which is almoſt double the number they were ten years ago.

The many interruptions to which a practical phyſician is conſtantly ſubject, muſt plead an apology for the inaccuracies, &c. committed by, Your's, &c.

M. D.

*** As it is probable, from what is obſerved in the preceding pages, that the Bar of the New York Repoſitory will be ſhut againſt Dr. Patterſon's defence, and that he will be obliged to apply for a hearing to the judicatory of the London Medical Journal; he relies on the candour of the Editors of this impartial print, that they will not follow the example of the American Editors, who ſeem to have uſurped the privilege of the Hibernian tribunal, with whom it is ſaid to be the faſhion, " to *condemn* a man firſt and to put him on *trial* afterwards."

To the Editors of the Medical and Physical Journal.

Gentlemen,

If you think the following obſervations on the Fever which raged in Aberdeen, deſerving a place in your uſeful Publication, I ſhall thank you to inſert them.

I am, &c.

Aberdeen, Feb. 27, 1802. TUAAM PEAAL.

During the months of November, December, and January, fevers were very prevalent in Aberdeen; the weather in theſe months was very ſtormy, and the froſt intenſe, yet it had not the effect either of diminiſhing their malignity, or ſtopping their progreſs. Typhus and its varieties, eſpecially that kind called Synochus, or a mixture of Synocha and Typhus, are, according

according to Dr. Ferguson, the species of Fevers which most commonly affect the inhabitants of Aberdeen. He observes, that every country has its own particular species of Fever, which depends upon the nature of the place, and temperaments of the inhabitants; and this difference, he says, appears in the series and malignity of the symptoms. The Fevers which commonly occur here, when they do not proceed from specific contagion, can be attributed to no other remote cause than cold, which has by several authors been considered as the most frequent cause. *This may account for Fever appearing more frequently in winter and cold weather, than when it is temperate and warm.*

The air round Aberdeen is in general pure and dense, free from fogs and marsh miasmata; yet it frequently abounds with marine vapours, especially when the wind is easterly, which is the cause of sudden vicissitudes of heat and cold. This produces sthenic diathesis, and therefore frequently subjects the inhabitants of this city to catarrh, rheumatism, pneumonia, and other diseases, the effects of a changeable atmosphere. Their food may also contribute to the same effect, being dense and coarse; consisting principally of corn meal, which is very heating, and requires a strong and healthy state of the stomach and chylopoietic organs to reduce it to chyle. The cool air of this place, along with exercise, is however very favourable for assisting the digestion not only of the kind but also the quantity which is generally taken; and were they to be fed with food lighter, and more easily digested, they would complain of hunger in less than half an hour. Intermittents and remittents do never occur here. Dr. Ferguson, in his Essay on Fever, has also remarked this, we may therefore infer that not only the fevers which rage here, but their causes, are specifically different from that of intermittent fever; and as a further proof of it, the air of this place generally proves a cure for the latter. Dr. Brown says, that in Typhus there is great accumulation of excitability, want of excitement, and deficience of the vital powers or defective stimuli. Dr. Cullen, that there is a great degree of spasm, and violent re-action. Dr. Ferguson thinks the debility is owing to the incapacity of the system for receiving and retaining oxygen, which he says is diminished in Typhus. But we must confess that, notwithstanding the light which these authors have thrown upon the subject, we yet remain in darkness. It is true, we are possessed of a medicine that will generally stop the paroxysm of intermittents, but we are not yet in possession of any medicine that will arrest the progress of the continued paroxysm. Has experience yet furnished us with a proper method of cure that will answer in every case of the same kind of fever? Digitalis, mild muri-

ated mercury, acids, and oxygen have been much recommended; yet, when tried, the advantage has not been greater than that attending the former methods of treatment. Neither is this to be wondered at, confidering that all the functions are fo violently affected in continued fever, that we are at a lofs to form proper indications of cure. The manner in which the caufes operate are hid from our view, and what is taken for caufes are only effects of the caufe. On this fubject a wide and extenfive field for improvement lies open. Nothing tends more to forward our knowledge of fever than having an exact account of the nature, caufes, and cure of fuch fevers as are endemic in particular places. By this we have already learned that contagion and cold temperature, operating upon the inhabitants of certain places, produce fevers of the typhoid kind; and that in warm climates, contagion and heat operating will produce a fever of a different and more infectious nature, like the bilious remittent fever. It is known too, that marfh miafmata, acting with cold, will produce a certain fpecies of intermittent; when acting with heat it will produce a different kind; hence the well-known diftinction of intermittents into vernal and autumnal. Dr. Fergufon, with great juftice, confiders that cold and heat frequently act as remote caufes, and that excefs of heat or cold obftructs the proper oxydation of the fyftem. Whether or not this be the cafe, we find cold acting in excefs to have been the moft frequent caufe of the fever which raged in Aberdeen during the paft winter. It moft frequently affected the young and growing; it commenced with languor and chillnefs, flight pain in the knees, fmall of the back, and forehead. The fenfation of cold was fometimes very violent, but feldom produced tremor. From the beginning there was great diminution of ftrength, attended with anxiety and dejection of fpirits. From the weaknefs of the mufcular action of the thorax, the patient complained of oppreffion at the breaft, accompanied with frequent fighing, and an averfion to fpeak. They complained of a fenfation in their ftomach as if they had eaten an over meal; this was attended with more or lefs ficknefs, naufea, and fometimes vomiting. Thirft, drynefs of the fkin, fcanty and high coloured urine likewife attended. The chillnefs generally continued two or three days, and was fucceeded by a warmnefs of the fkin, which increafed to a burning heat over the whole body, fenfibly felt by every perfon who touched the patient; and if any perfon flept with him he felt a difagreeable fenfation of a dry intenfe heat, as if the patient's fkin were burning him. This heat was very fevere, and continued for a longer or fhorter period according to circumftances. It often continued four or five days

days without any ſign of ſweat appearing, and during this ſtate of heat the patient felt himſelf as uneaſy as in the ſtate of cold. There was alſo a ſlight redneſs of the adnata of the eyes, attended with throbbing of the temples, furred tongue, quick pulſe, and reſpiration. Sometimes a kind of moiſture aroſe upon the ſkin, but it more frequently was dry and parched. This ſweat ſeldom gave any relief to the ſymptoms, and when it was profuſe it generally tended to weaken the patient more. Delirium, in ſome caſes, came on about the ſixth or ſeventh day, in others not till the tenth. This ſymptom was very variable, ſometimes it was violent, and at other times it occurred only during ſleep; it was accompanied very frequently with ſubſultus tendinum, difficulty of uttering words, and comatoſe ſtupor. Theſe ſymptoms, however, only attended the worſt caſes. When the delirium was violent, if it did not abate within three days, the patient ſeldom recovered. The criſis happened about the 15th, 16th, or 17th day. When favourable, the pulſe became ſoft, regular, and not ſo quick; the exceſſive heat and thirſt abated, and a moiſture appeared on the ſkin; the delirium and ſubſultus tendinum alſo abated, and the patient was more inclined to ſleep. When unfavourable, the delirium and comatoſe ſtupor increaſed, the pulſe turned very quick and feeble, the patient toſſed much and ſometimes cried, would not take drink, became incapable of ſpeaking, and at laſt death cloſed the ſcene. This is the manner in which the fever began, proceeded in its courſe, and terminated either in health or death. Sometimes the ſymptoms would vary a little, but in general the deviation was very inconſiderable. A diarrhœa would in ſome caſes appear and prove troubleſome, ſometimes alſo ſlight hemorrhage from the noſe; but theſe ſymptoms did not always attend, nor did they ever prove favourable.

The moſt ſucceſsful method of treating this fever was the following: When the patient applied for aſſiſtance in the beginning of the fever, or during the time when he felt his body chilly and cold, he was ordered to bed, and a cup full of warm water, a little ſweetened, given him every hour, in which was mixed ten gts. of a mixture of equal parts of vinum antimonii and tinctura opii, until he began to perſpire; at the ſame time a bliſter was applied to each temple, which always had the effect of removing the pain of the head. After twelve hours, if this treatment had not produced a gentle perſpiration, but the burning heat ſtill continuing upon the ſurface of the ſkin, the patient was bathed with cold water, his body well dried, and an anodyne draught preſcribed. The drink was now changed to weak negus, with the addition of half the quantity

formerly prescribed of the vin. antimon. & tinct. opii. If these means were attended with the effect of keeping up a gentle perspiration, nothing else was given excepting chicken or veal soups which were used as food. When, however, the fever, burning heat, sickness, quick pulse, stupor, and delirium increased, the quantity of wine was augmented, and stimulant cataplasms applied to the feet, such as recommended formulæ 241 by Dr. Ferguson, with the following bolus from the same author repeated every four hours until the fever begins to abate. ℞. G. camph. succinat potasiæ aa gr. iij elect. theb. gr. v. ft. bolus.—When cough proved troublesome the sevum ceti mixture was had recourse to. When diarrhoea was obstinate, attended with a dry and hot skin, affusion with cold water, giving at the same time small doses frequently repeated of the tinct. opii, was found very beneficial.

A few additional Remarks on the Identity of the Electric and Nervous Fluids, by T. Purton, *Member of the Royal College of Surgeons, in London.*

To the Editors of the Medical and Physical Journal.

Gentlemen,

At a time when the minds of so many Physiologists are engaged in the investigation and discoveries of the various phænomena of electricity, I think every man is bound to come forward with whatever might add to the support of, or will in the least tend to unravel, a subject which, as yet, is involved in so much obscurity. How far the following additional remarks to those which I have before written,* may be considered as a farther elucidation to those already advanced, I must leave others to determine; but they may possibly convey such an impression to other minds as may be productive of future discoveries, and ultimately bring us to the truth.

My principal reason for troubling you with any further remarks on this subject, was occasioned by the perusal of a pamphlet

* Medical and Physical Journal, Vol. iv. p. 334, et seq.

phlet published by the Rev. John Lyon, describing a very interesting account of the effects of lightning on the bodies of a man and four horses, which were killed near Dover in Kent. The phænomena were truly interesting, for in three of the horses the right ventricles of the heart were ruptured. In the other horse the carotid artery on the right side was ruptured, but the heart was not apparently injured. It is much to be regretted that the man was not opened.

Mr. Lyon, I think, proves to a demonstration, that the blood and not the nerves, was the conductor of the electric fluid, which certainly is not an inconsiderable addition in support of my general argument. Mr. Lyon does not appear to have understood much of the circulation of the blood, or he would not have suggested the idea that the electric fluid was conveyed by the arteries to the right side of the heart, which is not only directly contrary to the course and motion of the blood, but its termination in that case would have been into the left ventricle. I cannot attribute the immediate death of the horses to the rupture of the ventricles, as I have not any doubt that this event would have been as instantaneous if the whole vascular system had escaped uninjured. The death of all animals from lightning, is, in my opinion, fully established by facts and experiment, to be caused by the concussion and sudden shock given to the brain; as to the accidental rupturing of the hearts in these instances, it may be considered as a singular occurrence, owing probably more to the favourable position of the man and horses to transmit the electric matter, than a general effect in all cases.

If the arterial and venous circulation, which I have hinted at in my Paper on the physiology of the brain and nerves, are the collectors of the electric fluid, the phænomena attending the death of the horses may be better explained; for the electric matter was passing in the same direction as it does in the usual course to the common reservoir the brain. Time may determine how near I am to the truth; but I really think the course which the electric matter took, joining itself with that already accumulating in the body, strengthens very much my general doctrine. Is it at all improbable, that this penetrating matter, which by some peculiar combination or modification is to produce the various phenomena in the nervous system, may be accumulated through the medium of the circulation, being derived ab externo, by those innumerable pores with which the whole surface of the skin abound, but principally by respiration? I cannot help thinking that the oxygenation of the blood is in some measure connected with this very wonderful matter, either chemically or otherwise, in the circulation through the lungs; as

as it is probable that the ſenſation of the animal depends entirely upon the influence of reſpiration; for we may find by daily obſervation that ſenſibility ſeems to be impaired exactly in proportion as this function becomes defective, until, at length, they both are extinguiſhed nearly at the ſame inſtant of time. How far the relation between oxygen and the electric fluid will bear an inveſtigation is a matter worthy of the conſideration of future phyſiologiſts, and alſo the changes which evidently take place in the very properties of the blood, on the firſt ruſhing of the air into the cells of the lungs; probably the electric fluid owes its firſt principal modification to this grand union with oxygenous matter. Theſe are certainly circumſtances which require a very attentive reſearch, as the reſult of our inquiries may alſo point out, what conſtitutes the diſtinctive differences between fœtal and adult life. I think this cannot be denied, that one of the principal uſes of reſpiration, is to keep up a certain energy or healthy organization in the brain and nerves, which qualify them ſo eminently for the various faculties of perception and ſenſation. This energy, probably, is nothing more or leſs than that the nerves and their appendages ſhould conſtantly poſſeſs a certain ſpecies of electric influence, over and above what is accumulated in the brain for the different purpoſes of the animal; ſo that their remote and exterior ramifications ſhould be kept in that continual ſtate of ſuſceptibility which is eſſential to health.

The means which are uſually found effectual in reſuſcitating perſons apparently drowned or ſmothered, infants ſtill-born, &c. &c. appear to favour this theory; we firſt of all produce action in the heart, but ſenſation probably does not take place until the functions of the lungs are reſtored. The ſtate of the nerves before and after birth is certainly very different; they ſeem in utero to be merely the agents of motion, but after the blood has received the benefit of reſpiration they immediately perform a much more important function.

The attrition and velocity of the circulation appear to me in every way ſufficient for the purpoſes of accumulating the electric fluid; which, probably, after it has undergone ſome other changes in its circulation through the lungs, conſtitutes that energy which we call nervous. The placenta acting as the fœtal lungs, is in every way calculated to produce ſimilar changes in utero. The brain encloſed by its membranes and bony compages, might a priori be conſidered as a proper reſervoir for matter ſo ſubtle and diffuſive in its nature. In the caſe of Dr. Monro, no doubt that the ſpinal marrow encloſed by the theca vertebralis performed a ſimilar office in utero; and ſuppoſing a child was born without a brain, ſpinal marrow,

and

and even a heart, of which there have been inſtances, the arteries and nerves under the above theory would have anſwered every purpoſe. Therefore, under this idea, the nerves have nothing to do with the office of accumulating the electric matter, but of tranſmitting the ſame, after a certain modification, to all parts of the body, for the purpoſes of ſenſation and muſcular motion.

The courſe of the circulation being ſurrounded by the beſt non-conductors, may be conſidered as acting ſimilar to the glaſs cylinder in an electric machine, and the brain as the Leyden phials; and how could nature ſuggeſt a more admirable contrivance either to confine or prevent a diſperſion of this fluid, or to conduct it more rapidly, than is found in the human body? Where is there a better non-conductor than the fat? and, on the other hand, where can there be found better conductors than the nerves and blood?

If it ſhould be thought that I treat a ſubject of ſo much importance too lightly, by comparing the nervous ſyſtem to an electric machine, I can only ſay, that it is far from my intention to do it; but in a matter of ſo much obſcurity, how can we poſſibly arrive at the truth, unleſs all our reaſoning and experiments are grounded upon an analogy with well eſtabliſhed facts.

If the preſent Inquiry ſhould terminate in giving place to a theory ſtill more defenſible, it muſt ſurely be to ſomething which will bear a very great analogy to the matter of electricity; for in how many points do theſe two very ſimilar fluids agree? yet there are ſtill differences which, I muſt acknowledge, are at preſent not explicable; but I entertain the moſt flattering hope that the time will ſoon arrive when this ſubject will be far better underſtood. After a very principal part of the circulation of the blood was found out, a conſiderable ſpace of time elapſed before the whole was completely diſcovered; and when we conſider what a little was left to be done, our ſurpriſe muſt be great indeed, to find, that nearly a century paſſed before the diſcovery was perfectly eſtabliſhed.

There are many natural productions, which bear ſo cloſe a reſemblance to each other in their very component parts, that we cannot help proclaiming them modifications of one and the ſame principle. May not the electric matter be ſo modified and changed by its circulation or otherwiſe, through the various parts of the body, as to be totally altered in many of its conſtituent properties?

My mind is impreſſed with ſuch conviction, that I cannot forbear thinking, we are in the right path to truth; therefore, let me expreſs a wiſh, that Phyſiologiſts will ſtill turn their attention,

attention, very earnestly, to this most important subject; and if any thing which I have suggested should be directly or indirectly the means of making it more clear, I shall consider my time well employed.

I am, &c.

Alcester, March 1, 1802. T. PURTON.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

AS a friend to the Medical Journal, I have experienced no small sensation of regret, in observing some late Numbers deformed by the virulence of personal hatred, a spirit equally unworthy the name of a Philosopher, and hostile to the advancement of Science and of Truth: I allude to the Communications upon Apoplexy, between Mr. Crowfoot and Dr. Langslow. This sensation I have felt more keenly, knowing, from some connection which I have with the eastern counties of this kingdom, that Mr. C. is held in high and deserved esteem in his neighbourhood, and that his professional knowledge and integrity are well known in the medical community in which he lives. If such is the case, it is much to be lamented that your various Correspondents, on this subject, who are strangers to the parties concerned, should hazard opinions and judgments of the value of their characters, and propriety of their conduct; as by such a proceeding, the feelings of the liberal and enlightened may be unjustly, though unintentionally, wounded.

The statement, as presented to the public, certainly does not convey that information which is necessary to form an accurate and candid judgment of the propriety of conduct of the respective parties; for it is to be collected from that statement, that Mr. C's letter was not written for the public eye, and that it was printed without his knowledge or consent; it may also be observed, that any difference of opinion between him and Dr. L. concerning the use of emetics in Apoplexy, was not the cause of their disagreement, but some previously existing motives had determined Mr. C. against an interview with Dr. L. as had been previously made known and mentioned to the husband of the lady. It is possible these motives may have justified Mr. C. in refusing an interview.

I have made these remarks in the spirit of candor, and in the hope that they may tend, in the future progress of this inquiry, to

to recall the attention of the medical world from perſonal invective to the queſtion under conſideration; a queſtion of conſiderable magnitude, and deſerving the minuteſt inveſtigation.

Before I proceed, it muſt be here remarked that the queſtion is not, as ſeems to have been underſtood, whether bleeding or emetics are to be uſed in Apoplexy, for the uſe of blood-letting does not ſeem to have been denied; but whether emetics are at any time uſeful in Apoplexy? It is evident there are two ways of determining this queſtion; the one is by the weight of authority; the other is by reaſoning upon and determining the nature of the action of emetics upon the ſyſtem at large. And if we are to look to the tribunal of authority upon this matter, I muſt freely confeſs that it has been with no ſmall degree of ſurpriſe that I have obſerved Dr. L. raiſing ſo high the voice of triumph over his opponent; as if he ſtood alone and unprotected in his opinion, that an emetic may, at times, be uſeful in Apoplexy. Such an opinion, I truſt, has at leaſt the ſanction of authority, as will be proved in the ſequel; and he who would charge its abettors with ignorance, can be but little acquainted with the hiſtory of medical opinions.

I have no deſire to cite a long liſt of medical authors favoring the uſe of vomits in this diſeaſe; but it may not be foreign to the ſolution of the preſent Inquiry, to remark, that amidſt many it will be found that Riverius and Helmont, that Mayerne and Etmuller thought well of them; neither needs that man to bluſh who, on this ſubject, joins in opinion with the ſagacious the immortal Sydenham, the experienced Shaw, or the diligent Lieutaud. Indeed, ſo little acquainted ſeems Dr. L. with the opinions even of more modern authors upon the continent on this ſubject, that it is neceſſary to inform him that the judicious Selle, of Berlin, and the late Profeſſor Stoll, of Vienna, have actually made a triple diviſion of Apoplexy, referring one ſpecies of it to the ſtomach, and naming it gaſtrica from its ſeat; their diviſion is, therefore, into the ſanguinea, the gaſtrica, and the ſeroſa or maligna. It needs not be remarked, that in the gaſtrica, emetics are the principal agents of cure. I ſhall not ſwell this liſt of names any farther, enough having been ſaid to prove, that he who charges perſons holding ſentiments ſimilar and congenial with ſuch characters with ignorance, fails at leaſt in candor, if not in information.

I have dwelt ſufficiently long on the uſe of emetics, as warranted by authority, which, however, on medical queſtions, in oppoſition to Dr. L. I am ready to confeſs has exerciſed a controul ſo unlimited over the freedom of inquiry, as to have arreſted in many inſtances the progreſs of truth. I muſt moreover remark, that the vaunting manner in which Dr. L. has

ſpoken both of Mr. Crowfoot and Dr. Girdleſtone, in diſcuſſing this ſubject, ſeems but little authoriſed, from the very nature of the preſent inquiry; and feeble muſt be that mind, that is awake to any great ſenſe of merit and ſelf-gratulation, in merely being able to detail and put in force the common received notions in the treatment of a very common diſeaſe; and I have alſo very ſtrong doubts whether theſe gentlemen have retired from the field of controverſy, on this occaſion, from any ſenſe of their antagoniſt's ſuperiority, ſo much as from knowing that much doubt and obſcurity attach to the ſuppoſed advantage of any mode of practice in this diſeaſe; and from feeling that the temper in which the diſcuſſion has hitherto been carried on, is but little favorable to the developement of truth.

But to proceed. In turning from authority to the dictates of reaſon, in this matter, I would aſk, is it certain that from the action of a vomit upon the ſyſtem, there is an increaſed impetus or determination of blood to the brain? Or, ſuppoſing that there is an increaſed determination of blood, does it follow, neceſſarily, that a rupture of veſſels, or effuſion, would take place? It ſeems to me, in reflecting upon the effects of an emetic upon the ſyſtem, that the firſt circumſtance that aſſails the attention, is a great diminution or enfeebled action of the vaſcular ſyſtem in general; this is obſervable during the nauſea preceding the act of vomiting; in this ſtate there is great pallor of body, faint ſweats, the pulſe is feeble and unſteady, and the reſpiration is affected; evident marks of leſſened action in the heart and arteries. It will not therefore be aſſerted, by the enemies of emetics, that in nauſea the blood is ſent to the brain, either with greater force, or in greater quantity; on the contrary, it is reaſonable to ſuppoſe, that in this ſtate there is a ſmaller quantity of blood, and leſs forcibly, ſent to the brain. Hence, in this view, granting the exiſtence of fulneſs and diſtenſion of the veſſels of the brain, ſuch fulneſs, it is probable, might have time to be removed by the powers of the veſſels, while the blood, during nauſea, is ſent to that organ with leſs impetus, and in leſs quantity, from the diminiſhed arterial action. But it may be replied, if during nauſea there be diminiſhed arterial action, the ſame cannot hold good during the act of vomiting, in which the circulation of the blood is interrupted, and it is accumulated in the brain. But in this act I doubt whether this accumulation exiſts, and for the following reaſons: It is a well-eſtabliſhed fact that the motion of elevation and depreſſion peculiar to the brain, alternates with that of reſpiration; that is to ſay, that in expiration the elevation, and in inſpiration the ſubſidence, of the brain takes place.

Now

Now it being eſtabiſhed,* that the act of vomiting takes place rather in a ſtate of inſpiration, in which, from the free return of the blood through the heart and the lungs, the ſubſidence of the brain, and, of courſe, its diminiſhed vaſcular fulneſs happen, it ſeems probable, notwithſtanding the appearance of ſuffuſion and fulneſs of the face, that in the act of vomiting, the ſtate of the brain is rather that of depletion than plenitude; on this ground, therefore, I ſuppoſe, it may be doubted whether emetics are hurtful by increaſing preſſure on the brain.

And it may be farther obſerved in general, that as the blood is the proper ſtimulus and cauſe of action of the vaſcular ſanguiferous ſyſtem, and a ſtimulus by which its tone and denſity, as compoſed of living ſolids, are kept up and ſupported, it would ſeem that the ſtrength of any given veſſel or veſſels, is in a direct ratio of the force of that ſtimulus; or, in other words, of the momentum and velocity with which the blood moves; hence it appears improbable that an increaſed impetus of the blood ſhould rupture a veſſel; and that the appearance of blood or ſerum effuſed, depends upon other cauſes inherent in the veſſels of the brain, rather than upon the increaſed motion of the blood. This poſition receives conſiderable illuſtration from anatomical experiment; it does not happen by throwing a ligature over the principal artery leading to a part, or over a principal vein leading from a part, that ſuch a veſſel ſo treated is ever ruptured, or that the ſmaller arteries and veins in the neighbourhood, by having an undue quantity of blood thrown upon them, give way or are ruptured; and it is known, that by ſuſpending animals by a rope thrown round their necks, it is a difficult matter to kill them, providing an opening is made in the trachea, below the rope, notwithſtanding the preſſure upon the veſſels, and which is probably more felt by the veſſels carrying the blood from the brain, than the arteries leading to the brain. Theſe facts prove at leaſt that it is not common to rupture veſſels by increaſing the impetus of the blood.

This poſition receives farther illuſtration from reflecting upon ſome morbid phenomena, in which the action of emetics would be ſuppoſed to excite hæmorrhage, if ſuch were the natural or neceſſary effects of ſuch action. It is well known that emetics are frequently and repeatedly employed in diſeaſed ſtates of the lungs, and where a diſcharge of blood has previouſly and repeatedly taken place, and that in ſuch caſes the appearance of hæmorrhage is retarded rather than promoted by their uſe. And there have alſo been caſes † of perſons la-

* Vide Soemmering de Corporis Fabrica.

† Vide Heberden's Commentarii de Morborum Hiſtoria.

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bouring under frequent and alarming hæmoptysis, who have been sent to sea for their health, and who have never passed a day for months without strong efforts to vomit, and yet without any recurrence of hæmorrhage. And I have also known two cases of persons contending with epistaxis, in an unusual and highly threatening degree, and in which circumstances have made it necessary, fearing a fatal change, to remove the patients to about a mile's distance; and in these cases, in which perfect rest and cool air would have been enjoined as necessary, and in which the motion of the carriage would have been thought likely to increase the discharge; I say, contrary to expectation, and the fiat of authority, it has been finally put a stop to while the carriage was in motion.

These considerations seem, at least, to render it doubtful, whether even in cases where hæmorrhage has taken place, and where there has been a supposed tendency to rupture of vessels by increasing the impetus of blood, such tendency is not rather diminished than increased, by the increased tone and density of the vessels from such impetus; and they also seem to shew that emetics, if hurtful in Apoplexy, prove so from some other mode of action than that of determining to the head.*

I shall here dismiss the question of emetics, and should have proceeded immediately to the consideration of Apoplexy, as at all times dependent upon the effusion of blood or serum in the brain; an assertion hastily hazarded by Dr. L. and, as appears to me, at variance with sound analogy and the evidence of the best medical records: But before I proceed, I can but remark, that I think Dr. L. a little unfortunate at his attempts at ridicule, and to have proved at least, in opposition to some elegant writers, that it is not a test of truth; for having ridiculed a state of Apoplexy as connected with the stomach, he proceeds through the small and large intestines, and at length fixes his shafts in triumph, in the toes. Now this situation, in my opinion, is somewhat out of season and unfriendly; for I am willing to hope that Dr. L. is not so totally ignorant of the opinions of authors, even modern, as not to know that one species of Apoplexy has been thought to have some connection with the toes, the most common seat of gout; and hence Burserius in his Institutiones Practicæ, has given us an Apoplexy a metastasi materiæ arthriticæ.

Dr. L. will excuse me, if I point out another inconsistency in his resolutions, if not in his reasoning; for with great anxiety for the promotion of medical knowledge, and of that of the

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* That which P. has admitted, p. 330, the impediment to the return of the blood from the head. Ed.

the diſeaſe in queſtion, he has reſolved upon a publication upon the ſubject, to facilitate to the young and inexperienced, a diſtinction in his opinion of the greateſt importance in practice; that is to ſay, the mode of diſtinguiſhing ſanguineous and ſerous Apoplexy. Now, at the ſame time that he holds forth the importance of this diſtinction, he confeſſes that blood-letting is not only neceſſary in ſanguineous, but in ſerous Apoplexy. This being allowed, it is difficult for me to diſcover the very great importance of the diſtinction; for if diſeaſes thought to be ſo different are to be treated by the ſame means, it will require no common logic to prove the neceſſity of the knowledge of ſuch difference. I wiſh theſe remarks to be conſidered as applying to Dr. L's ſenſe of the neceſſity of ſuch knowledge, and not as in any degree advancing any opinion concerning ſuch diſtinction.

Perhaps, it may relieve Dr. L. from his ſolicitude in publiſhing, when he is told, that even Morgagni has admitted bleeding in ſerous Apoplexy; and from a conviction that the diſtinction referred to is not ſo neceſſary, he may be alſo told, that Portal has lately publiſhed in France, two Memoirs on Apoplexy, the tendency of which is to prove, that the diſtinction, which Dr. L. thinks ſo important, is not only futile, but fatal, as, in his opinion, both forms of Apoplexy require bleeding and a ſimilar treatment. And I may alſo mention, that Hippocrates and the other ancient phyſicians had no diſtinction of this nature, dividing Apoplexy only into the levis and fortis.

I ſhall now proceed to lay before your readers, and Dr. L. ſome doubts which have ariſen in my mind, from reflection, experience, and reading, on the exiſtence of effuſion in all ſtates of Apoplexy. And being aware, that Dr. L. pays the greateſt deference to the authority of writers on the ſubject, I will take the liberty of introducing to his acquaintance, a few names ſupporting the opinion, that Apoplexy has exiſted without effuſion. Theſe writers, I flatter myſelf, even Dr. L. will confeſs are adequate judges of real Apoplexy; and he will not, I think, either as a philoſopher or a phyſician, be aſhamed to hold opinions conſimilar with them. The perſons I allude to, are, the natural hiſtorian and phyſician Valliſnerij, who has given ſeveral diſſections of perſons dying apoplectic; and where, upon examination, to the ſurprize of every bye-ſtander, effuſion of no kind, nor any morbid appearance of the brain, preſented itſelf. Alſo I beg leave to cite to the ſame end, Valſalva and Morgagni,* both of whom have acknowledged the

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* Vide Adverſaria.

the existence of Apoplexy without effusion, or any morbid appearance of the brain.

I have also seen two cases of persons of very spare habits, about 60 years of age, where there was not the least appearance of redundance of fluids, either serous or sanguineous, in the system, and who died strongly apoplectic. And moreover, if to the above facts we take into consideration the advanced period of life in which Apoplexy, for the most part, takes place, we shall find perhaps, in the natural history of old age, some changes and phenomena favorable to the generation of that diminished, nervous, or sensorial power, which forms the leading character of Apoplexy. In old age, it is a fact well established, that there takes place throughout the solids of the body, a diminution of vascularity, or, in other words, a conversion of living into simple solid; thus, in this season of life, the bones and muscles as well as other parts abound with fewer vessels; and this diminution of vascularity becomes greater, the greater the age. Now if this be admitted, the same changes which take place in other parts will also have their influence in the brain; hence in that organ, from a diminished vascularity, and of course sensorial power, or from a conversion of living into simple solid, a torpor is generated, favorable to that eclipse of nervous power or sensibility in which Apoplexy consists. And this loss of vitality of the brain appears to me a more probable cause of Apoplexy, than any pressure from fluids either effused or accumulated in the vessels. I have already, in speaking of emetics, attempted to shew, that a rupture of the vessels of the brain is less likely to take place from any increased distention or impetus of blood, than from a weakened state or tone of vessels, from a weakened circulation or diminution of impetus; similar to what takes place in the state of vessels in scurvy, and in the petechia and vibices of some fevers. Dr. L. also, in speaking of the distending power of the blood and the consequent rupture and effusion of fluids, seems to reason from the old tenet of the schools, that the volume of the blood is equal to, and in some cases even greater than the sum of the cavities of the vessels, and on that account he supposes injurious by over distending and occasioning rupture or effusion; but let Dr. L. recollect, that in no instance either in health or disease is the volume of the blood equal to the collective areas of the vessels; for upon killing any animal, in the fullest and best health, it will be found that the arteries are wholly empty, and that the blood in the system will barely fill the veins; upon this view then, effusion cannot arise from over-distention, neither is the indication for bleeding to be derived from that cause. It must also be confessed, by every candid inquirer, that in dissections of the human

human body, no problem is more difficult of solution than that of determining what appearances are the precursors or causes of disease, and what are the sequelæ or effects. I would also ask in dissecting apoplectics, granting that in the brains of seven out of ten there is found an effusion of blood or serum, or of both, are there no circumstances attendant upon the corpses of apoplectics favorable to such appearances? Observation and reason tell me, there are some peculiarities, in such bodies, favorable to such effusion, as sequela or effects of disease: For in the bodies of apoplectics, within twenty-four hours after dissolution, there is for the most part observable a great evasation of fluids of the body, which following the general law of gravity, settle to the lowest parts; hence it happens, that in examining such bodies, there will be found a great collection even of extravasated blood along the neck, the shoulders, the back, the nates, and the thighs; these parts, from the horizontal position, being undermost. And if the vessels in such bodies have so little power of retaining the fluids in those parts, what just reason can be assigned for the vessels of the brain having a greater power? And why, in that organ, should not a similar effusion take place after death, from the law of gravity acting upon the fluids? This effusion, it may be remarked, should take place in the brain in a greater ratio, that viscus being supposed to contain a tenth of the blood of the whole system, a greater portion than is contained, perhaps, in any other equal part.

These considerations have awakened in my mind, strong doubts of the existence of fulness of vessels or of effusion in the brain as the cause of Apoplexy; and doubts, whether such appearances are not with more reason to be considered as effects.

But let it be conceded to Dr. L. that effusion either of blood or serum does actually exist as a general cause of Apoplexy; there is still a small difficulty awaiting this supposition, which he does not seem aware of, and which is, where are the vessels of the brain by which such effused fluid is to be absorbed? Where are the lymphatics? And who has pointed them out either by the faithful test of injection, or by appealing to the senses by dissections that cannot be doubted or denied? An answer to these questions is important to Dr. L.: because, whether five ounces, or five tea spoons full of fluid be thrown upon the brain, it must unceasingly remain there, unless these questions can be duly answered.

On this subject I have attended to the writings of a Haller and a Hewson, of a Cruickshank and a Monro, of a Caldani, a Mascagni, and a Lupi; and I can find in them nothing satisfactory on this head, or little more than idle conjectures and vague analogies.

This being the case, it has been matter of much wonder with

with me, that Dr. L. ſhould have made the abſorption of ſerum or blood effuſed in the brain (an organ not known to poſſeſs abſorbents) ſo eaſy and ſo quick a proceſs; while, for the moſt part, in other organs, and poſſeſſing abſorbents, it is a proceſs requiring conſiderable time.

These arguments are ſufficient, at leaſt, to render uncertain the exiſtence of preſſure of the brain, from the effuſion of blood or ſerum, as a general and neceſſary cauſe of Apoplexy; and perhaps they throw ſome doubt upon the utility of blood-letting in this diſeaſe; which is, I think, farther confirmed by the following remark, That I have ſeen many caſes of Apoplexy, moſt of which have been treated by venæſection; but I confeſs, I have ſtrong doubts, from a compariſon of them, whether thoſe that have loſt the greateſt quantity of blood have not proved the moſt fatal.

Laſtly, I ſhall beg leave to ſuggeſt to your readers and Dr. L. that it is probable the ſeat of the cauſe of Apoplexy may be frequently placed in the lungs,* rather than the head. From the advancement of Phyſiology, and a more intimate knowledge of reſpiration, a perpetual renovation of the air in the lungs is a proceſs eſſential and neceſſary to life, and to the action of the heart and arteries; as by this proceſs animal heat is generated, and oxygen is conveyed to the ſyſtem at large, a principle widely diffuſed in the animal œconomy, and acting an important part on the irritable and ſenſorial ſyſtems. Now if it can be admitted, in old age, the ſeaſon of Apoplexy, that the lungs become leſs pervious to this principle, and leſs ready conductors of it to the ſyſtem, it may thence follow, that Apoplexy, or diminiſhed irritability or ſenſorial power, may take place. This ſeems, in a degree, confirmed by writers, who have at times connected the frequency of Apoplexy with certain ſeaſons or ſtates of the atmoſphere; as may be ſeen from Cole, Etmuller, and others; and alſo from the hiſtory of a family deſtroyed by the fumes of charcoal, as mentioned by Portal; in whom, upon diſſection, not only diſtenſion of veſſels had taken place upon the brain, but there was effuſion, as in *true Apoplexy*.

To conclude, the table of doubts here preſented to the public eye, is the reſult of my reading and reflecting upon this ſubject; and permit me to ſay, that I have little ſolicitude or anxiety about the reception of thoſe doubts; for ſhould they be removed and refuted, the cauſe of truth will be ſerved by the removal of error. And I ſhall be ſufficiently repaid for compoſing

* We ſhould wiſh to preſent our readers with ſome inſtance of this. Ed.

posing this table, if by shewing, that considerable shades of doubt and uncertainty hang upon this subject, I shall have repressed in the Mover of this Controversy, boldness of assertion and acrimony of invective; and shall have begotten in him temperance and forbearance, in the investigation of this subject, in which all parties must confess, there is much difficulty and uncertainty. And from a sense of this uncertainty, I can but regret, that I cannot accord with Dr. L. in his opinion, that Apoplexy is to be removed with a facility equalling in power the fascination of a Mesmer; or the enchantment of a Perkins; and I must candidly confess, that it is to be doubted, whether medicine has ever done much towards its removal, whether it has been treated by bleeding or vomiting, by purging or blisters; and I feel no reluctance in admitting the truth of the axiom of Hippocrates, when he says, "Apoplexiam solvere vehementem impossibile; debilem non facile." And on this occasion, perhaps, may be applied to the human body, a remark which the sagacious historian Tacitus has applied to the political, and who says, "Cauta potius consilia cum ratione sequenda; quam prospera interdum, ex mero casu fortuito."

PYRRHO.

On Acetous Ablution in Typhus and Synochus, &c.

To Dr. BRADLEY.

DEAR SIR,

THE internal use of vegetable acids was well known to the ancients; and they were held by them as a beverage highly salutary, not only in diseases where a putrescent and dissolved state of the blood and juices was supposed to exist, but also as a common drink in health, to guard against the accession of such diseases. Thus we find the Roman labourers and soldiers had a daily allowance of meagre wine, or vinegar and water, called *posca* or *lora*. (See Varr. Cato, Pliny, and others.)

The exhibition of vegetable acids was equally known and adopted by the physicians of subsequent ages, and has been continued to the present day in many diseases which shew a tendency to putrefaction, as in the scurvy, &c. That however produced by fermentation, seems better adapted to medical purposes than the native acids, for it is known to arrest or prevent

vent the procefs of fermentation, and is admirably calculated to remove that alkaline ftate of the fluids which is found to exift in fome perfons even in health, and in many cafes of difeafe. The *acetum vini* is the moft powerful of thofe produced by fermentation, and that on which I am now to deliver my fentiments. When taken into the ftomach in moderate quantities it is well known to promote digeftion, and even to prevent thofe flatulencies which might otherwife arife from the decompofition of the vegetable part of our diet. It is equally certain that when taken to a given extent it promotes the falutary excretions, and when in greater quantity becomes a powerful refrigerant. Upon a due confideration of thefe well known effects, together with what had been produced by the affufion of water in febrile difeafes, I was led to fubftitute the *acetum vini* as poffeffing an advantage over water by reafon of its greater antifeptic powers, and to prefer frequent ablution to affufion. This treatment I have directed in many inftances of typhus, fynochus, &c. both in military and private practice fince the fpring of 1794, with the moft marked advantage, and in which a happy refult not only confirms the beneficial influence of this acid, beyond any other medicine with which I am acquainted, but has led me to confider it as meriting the general attention of the profeffion, not only in thefe difeafes but in all others which by their analogy may afford the fame obvious indications of cure. That your readers may judge for themfelves, I have felected two cafes, which are recent. In the firft, it will be feen that the ablution produced effects equally rapid and beneficial, nor could the quantity of bark thrown into the inteftinum rectum be fuppofed to produce much effect in fo fhort a time. The cafe, however, will fpeak for itfelf.

Sarah Taylor, aged about twenty months, was feized with fymptoms of pyrexia on the 25th of October laft. Having enjoyed good general health from her birth, and not having been expofed to the atmofphere of any perfon labouring under contagious fever, her father (a refpectable furgeon and apothecary) gave her a gentle aperient, and diluent febrifuge medicines. The fymptoms however gained ground, and on the 27th, excited confiderable alarm. I was requefted to fee the child, whom I found with every fymptom of exquifitely formed typhus, all of which were heightened by fuppreffion of the natural excretions. I prefcribed $\frac{1}{16}$ of a grain of antim. tartarizat. to be given every hour until an evacuation fhould be procured, after which an injection of extract. cinchonæ Americanæ ʒj. with decoct. cinchonæ ℥iv. was thrown up and repeated every fixth hour. On the following day I faw her again, when vibices were found to have occupied the anterior, pofterior, and

exterior

exterior ſurface of each foot, and extending a conſiderable way up the leg; they had aſſumed the livid hue and the extremities were cold, and the cuticle had begun to ſeparate on the ſuperior part of the great toe. We agreed in opinion that little or no hope was to be entertained of her ſurviving many hours. The anxiety of the parent was extreme; he conſidered every effort we might make to ſave the child abortive, and only tending to harraſs the little ſufferer. My hopes were but little more flattering; yet I preſſed the continuance of the injections, and that the child ſhould be ſubmitted to *frequent and copious ablution with vinegar*. My advice was complied with by the father, rather I believe through compliment to me and a wiſh to leave nothing undone, than from any hope he entertained of its even mitigating the ſufferings of the child; and the mother, a woman of great good ſenſe and fine feelings, catching a ray of hope from the propoſed plan, performed the ablutions herſelf, frequent and copious, throughout the night. The following day ſurprized us with evident amendment by the diſappearance of the vibices. The ablutions were continued without any other collateral aid, but that of wine and nutritive diet; her return to health was effected in a few days, and her convaleſcence has remained complete to the preſent hour. In tracing out the cauſe of the diſeaſe, we could fix upon none other than infection or contagion from *fomites*, the father having, two or three days before the ſickening of the child, returned from viſiting a patient ſome miles off who then laboured under ſcarlatina anginoſa, and without taking off his Spencer, began nurſing the child. If this were the caſe, why had not this child the characteriſtic appearances of ſcarlatina? Does not this caſe ſeem to prove that contagion from one diſeaſe is capable of producing a diſeaſe different from, and wanting the pathognomonic ſigns of that from whence it derived its ſource? The reſt of the family eſcaped infection.

I. King, jun. 23 years of age, by occupation a farmer, had laboured under rheumatiſmus acutus early in laſt autumn, from which he had been relieved. On the 25th of December following I was called in; he had been confined for about fifteen days under the care of Mr. Brown, an apothecary in the neighbourhood, on whoſe judgement of the caſe I could place the moſt unlimited reliance. The ſymptoms from the commencement had been thoſe of *typhus gravior*. He had given calomel conjoined with the antimonial powder, and the bark liberally. The diſeaſe however had proceeded without remiſſion, though by gradual progreſſion, till the day on which I was called in. I found him in a ſtate which admitted but a very cautious prognoſis. The bowels were not free, although he had taken the

calomel and antimonial powder daily; his pulse 108, and feeble in the extreme; much head-ach, and occasional slight delirium. The tongue and teeth covered with a thick, dry, and black sordes; the fauces and tonsils much inflamed, and deglutition very difficult; urine pale, and small in quantity; cold colliquative sweats, and great prostration of strength. I directed a bolus to be taken every night, consisting of pil. saponis gr. viij. calomelas gr. jß. opii gr. ß. The cortex was continued as before or with but very little alteration, and he took Port wine with the citric acid, ad libitum. But my principal support was placed on the acetous ablution, which was repeated every third hour, so as to consume from four to six pints of vinegar in the course of the day and night. I saw him again on the 27th, when the pulse was at 80, and fuller withal. Bowels regular; tongue more clean and moist; inflammations of the tonsils, &c. subsiding; urine natural; no head-ach or delirium. A moist and natural perspiration had succeeded the second ablution, and he felt in every respect better. The whole plan was directed to be continued. On the 29th I found him advancing rapidly to a state of convalescence. The bolus was directed to be continued without the calomel, and the rest of the treatment as before. On the 1st of January I took my leave, desiring him to continue his bark and ablutions for a few days, taking his nutriment at frequent periods, but with caution as to the quantities he might take at one time. On the 18th he came to my house, a distance of seven miles, in perfect health, except a little weakness of the lower extremities. I saw him two days ago, and learnt from him that he had, since the 18th, pursued his accustomed avocations in the farm, and was in perfect health. The soothing effect of the ablution, applied cold, was such as will ever leave a lasting impression on his memory; and although much aid was derived from the cinchona, wine, and acid taken internally, I do not believe he would have recovered without the ablution.

It may be expected I should give some opinion as to the modus operandi of acetum in these cases. I am inclined to believe that its primary action is that of a stimulant, but in such degree only as may excite an healthy action of the solids, especially of the inhalents and absorbents. I believe in this way it is readily taken into the habit, and promotes the action of the whole vascular system. That its powers are antiseptic and astringent there can scarcely be any shadow of doubt; and that it may form a *tertium quid*, destructive of contagion, I am led to infer from repeatedly, I might say uniformly observing, that, cæteris paribus, where it had been liberally used, the influence of the contagion has appeared to be very limited, or altogether destroyed

deſtroyed, inſomuch that actual contact would not produce the diſeaſe, where the adminiſtration of vinegar had been ample to the patient, his apartment, and every thing about him. I ſhall, in a few weeks, ſubmit to you ſome Obſervations on the efficacy of Elaterium and Digitalis in certain diſeaſes. In the interim, I am, &c.

9th March, 1802. HUGH MOISES, M. D.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

MR. Cooke's practice of waſhing patients affected with contagious fever, as well as perſons expoſed to the riſk of contagion, with vinegar, cannot be too generally adopted. I have no doubt it will give him much pleaſure to be informed that a ſimilar mode of treatment was followed, with the moſt marked ſucceſs, in the plague which occaſioned ſo much havoc in Ruſſia, in the year 1771. In various parts of his Treatiſe, De Mertens adviſes ablution with vinegar; and in his account of the means by which he kept the great Foundling Hoſpital at Moſcow free from the peſtilence, he particularly mentions that he directed the "children who were brought (from infected families) to the quarantine-houſe, to be ſtripped to the ſkin; after which their clothes were burnt, *their bodies waſhed all over with vinegar and water*, and new clothes put upon them."

March 13, 1802. ANTILOIMICUS.

Obſervations on the Modus Operandi of Opium.

[Continued from Vol. VII. p. 137.]

AS I have not repeated the experiments with which I concluded my laſt paper, I ſhall only obſerve, at preſent, that ſhould they be confirmed by future experiments, we ſhall be authorized to conclude, that opium, introduced into the ſyſtem, through the medium of the abſorbents, operates *directly* as a ſedative.*

I am

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* I find by the following note which occurs in p. 33, of Dr. Wilſon's Eſſay on Opium, and which I muſt have overlooked at the time, that Dr. Alexander

I am now to proceed in endeavouring to prove that opium does *not* act as a stimulant.

The 9th Experiment* is liable to the same objections as the 1st, 2d, 3d, and 4th.

Exp. 10.† "Through a very small opening, I injected into the cavity of the abdomen of a rabbit about half an ounce of the same aqueous solution, preventing as much as possible the admission of air. Twenty minutes after the animal was opened, and the surface of the intestines appeared much more red than usual, and apparently inflamed."

So far is this experiment from proving that opium is a stimulant, when *all* the effects which follow its exhibition in this way are considered, that it would have been extremely difficult to have adduced any thing more unfavourable to that opinion.

It has been proved by experiments made on an extensive scale, on different animals, and by some of the first Physiologists of the age, that by injecting a solution of opium into the cavity of the abdomen, the sensibility, irritability, and mobility are *diminished* in a very remarkable degree; effects which cannot be owing to a *stimulant* property in the medicine, as they are not preceded by any symptom of increased action or excitement. The *primary* effect it produces, is a *diminution* in the *frequency and force* of the pulsations of the heart and arteries; the animals soon become *torpid and languid*, and continue in this state as long as life remains.

The following experiment is taken from Dr. Whytt's Essay on Opium,‡ "The same young gentleman, Mr. Ramsay, at my desire, made the following experiment. On the 9th of April, 1755, after making an opening into the cavity of the abdomen of the dog on whom the last experiment was made, he injected by the wound a drachm of opium dissolved in two ounces and a half of water; but before he could stitch up the wound, about an ounce of the solution escaped. The dog lost the *power* of his hinder limbs almost *instantaneously*. Two minutes after the injection was made he began to be convulsed; and

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Alexander does not assert that opium applied externally is never absorbed, which might be inferred from a remark which occurs in my last paper. Med. and Phys. Journal, Numb. XXXVI, p. 125. "Dr. Alexander was led from his experiments on this head, to conclude upon the whole, that although we cannot positively assert that opium is not absorbed, yet we have no proof of its being so, at least to such a degree as to kill." I am sorry I did not find out the mistake in time to correct it.

* Inquiry, p. 54.

† Inquiry, p 54, 55.

‡ Ess. & Obs. Phys. & Lit. Vol. II. p. 327—329.

and in two minutes more, after having raiſed himſelf upon his fore-legs, he *fell down* ſenſeleſs. At this time Mr. R. laid bare the thorax, by diſſecting off the tegument, *which did not seem to give the dog any pain*; and could plainly feel the motion of his heart through the pleura; it beat 76 times in a minute, but became gradually ſlower.*

"Immediately after counting the pulſe, Mr. R. cut the ribs on each ſide of the ſternum, which he laid back in the uſual way. The heart, which was thus brought in view, appeared quite turgid, and continued in motion about five minutes; during which time it performed only between 60 and 65 *weak* vibrations; for they were not compleat contractions. While the heart was thus moving, warm ſaliva was firſt applied to it, then cold water, and laſt of all oil of vitriol; which ſhrivelled the parts it touched almoſt in the ſame manner as a hot iron would have done; but none of them accelerated the heart's vibrations, *which became gradually slower, till they ceased altogether*. The fibres of ſome of the intercoſtal muſcles on the right ſide of the ſternum, continued to be agitated with a weak tremulous motion near half an hour after the injection was made into the *abdomen*; but the intercoſtal muſcles attached to the ribs, on the ſides of the thorax, were not obſerved to move, nor did the diaphragm make any motion when its fibres were pricked or cut.

"*Nothing remarkable was seen in the abdomen*; only, although it was opened ten minutes after making the injection, the *intestines had no motion*; *whereas, in another young dog, which had got no opium, Mr. Ramsay observed the peristaltic motion continue half an hour after laying open the thorax.*

"The dog loſt little or no blood in making the wound into his abdomen, nor were any of his bowels hurt by it."

The experiments I ſhall next introduce are from Dr. Monro's Eſſay on Opium.† They are, if poſſible, ſtill more deciſive in proving the *directly* ſedative properties of opium.

"To determine this queſtion ſatisfactorily, it was neceſſary to diſcover ſome other way in which the animal could much more quickly be affected to a violent degree. This I found might be done by pouring thirty drops of the ſolution of opium, through a ſmall hole, into the cavity of the abdomen.‡

(SEVEN

* The dog's heart, in a natural ſtate, and before the injection of the ſolution of opium, beat 150 in the minute.

† Eſſ. & Obſ. Ph. & Lit. Vol. III. p. 317, 318.

‡ "Like to what Drs. Langriſh and Whytt had obſerved of lauro ceraſus and opium on dogs."

(SEVEN TRIALS.)

" For in *two* minutes thereafter *the heart did not beat above half its usual number of times in a minute, and after four or five minutes, not above a third part of the usual number*; and, on examining the foot with the microfcope, I found that the blood in its veffels HAD ENTIRELY CEASED FROM MOTION: And now the whole mufcles were repeatedly convulfed, and the limbs extended, fo as to render the animal *unable to move out of the place where it lay*.*

After a quarter of an hour it fcarcely difcovered any outward fign of life, though the heart was found to contract *feebly* about *ten times in a minute five or six hours thereafter*."

Dr. Wilfon has been at confiderable pains to afcertain in what manner opium acts in producing the abovementioned phenomena, and has happily fucceeded.†

I fhall only make fuch extracts from his Work, as are neceffary to confirm and elucidate the experiments I have juft cited.

" I therefore repeated Dr. Monro's experiment more than once, but found the refult as he has ftated it. *The beating of the heart became less frequent*, ALMOST IMMEDIATELY, *on injecting the solution into the cavity of the abdomen*." P. 58.

" Thefe circumftances led me to confider, whether or not there is any other way in which a folution of opium, injected into the cavity of the abdomen, can be fuppofed to influence the motion of the heart, befides through the medium of the nervous fyftem, or that of the abforbents.

" A conjecture occurred to me, which is confirmed by feveral of the following experiments, namely, that opium applied to the coats of the blood veffels, by deftroying their mufcular power, *(opium, it was found, destroys the power of action in all muscles to which it is immediately applied)* muft affect the circulation in thefe veffels; and, confequently, thrown into the cavity of the abdomen, influence the motion of the heart by impeding or entirely interrupting that of the blood, in nearly one third of the whole animal; by which the fupply to the heart is diminifhed, and a greater than ufual obftacle oppofed to its perfect evacuation.

" The firft circumftance, then, to be afcertained, is, whether opium applied immediately, or nearly fo, to the blood veffels of a living animal, impedes or wholly interrupts the circulation

* Frogs were the fubjects of thefe experiments.
† Effay on Opium, p. 57—69.

lation in thoſe veſſels, independent of any general affection of the ſyſtem?

"It is to be obſerved, that in the following experiment, the opium is not applied immediately to the coats of the blood-veſſels, but injected into a cavity, between which and theſe veſſels a denſe membrane of cellular ſubſtance is interpoſed. The ſkin of a frog, except in a few places, (chiefly the joints) does not adhere to the parts which lie beneath it.

"Having adapted the web of a frog's foot to a microſcope, I injected eight or ten drops of a ſolution (nearly as ſtrong as the ſtronger ſolution) under the ſkin of the leg.

"*In a few seconds the circulation became languid, and no motion could be perceived in some of the larger blood-vessels. It gradually became more obscure in the rest, till, in the space of about two or three minutes after the injection of the opium, it ceased altogether.* Nor did this interruption of the circulation proceed from any general affection of the ſyſtem, ſince the motion of the blood ſtill continued in the other foot.

"Leſt it might be ſuſpected, that in applying the foot to the inſtrument the veſſels were compreſſed, it is proper to obſerve, that the circulation in the foot, after it was applied to the microſcope, continued as vigorous as natural, till the ſolution was injected. On another occaſion, indeed, I have obſerved the circulation in the foot of a frog, applied in the ſame way to the ſame microſcope, vigorous for ſeveral hours.

"This experiment was made three times, in the ſame manner, and with the ſame reſult.

"After determining that opium is capable of interrupting the circulation in the part to which it is immediately applied, independent of any general affection of the ſyſtem; all that is neceſſary, in order to aſcertain whether it is in this way that it ſuddenly affects the motion of the heart, when thrown into the cavity of the abdomen, is to interrupt the circulation, and obſerve the effects of this drug, applied in the ſame way, when it can only act on diſtant parts, through the medium of the nervous ſyſtem." P. 59—63.

A particular account of the experiments immediately ſucceeds (p. 64—68.) The following is the concluſion they ſuggeſt.

"The inference from theſe experiments is, that the *diminished frequency of the heart's motion, observed almost immediately on throwing a solution of opium into the cavity of the abdomen, does not proceed from any action of the opium on this organ, through the medium of the nervous system, but from its impeding or entirely interrupting the circulation in nearly one-third of the whole animal.*" P. 68, 69.

Effects nearly ſimilar were obſerved to reſult from injecting opium into the abdominal cavity of guinea-pigs, by the Abbé Fontana.

His words are, "Thoſe into the bellies of which the aqueous ſolution of opium was injected, died in leſs than two hours: *They lost the greater part of their motion in less than half an hour*, and were violently convulſed."*

On ſumming up the evidence derived from the above experiments, it appears that opium injected into the cavity of the abdomen of different animals *deprives them of voluntary motion almost instantaneously*; *that it impedes or interrupts the circulation of the blood, in the vessels of the abdominal viscera*; *the pulsations of the heart almost immediately become feeble and slow, and the circulation of the blood ceases in the extremities*; *the peristaltic motion of the intestines is retarded and destroyed: The sensibility and irritability are impaired to such a degree, that the dog*, in Mr. Ramſay's experiments, *did not seem to feel any pain when that gentleman laid bare the thorax, by dissecting off the teguments, though the heart was beating* 76 *times in a minute at the time*; *nor did the diaphragm make any motion when its fibres were pricked or cut.*

Dr. Crumpe's view in making the experiment ſeems evidently to have been, to obſerve the *local appearances* reſulting from the application of a ſolution of opium to a tender irritable ſurface; and this accounts for his omitting to notice any other circumſtance. But after having *proved to a demonstration*, that the *primary and general* operation of opium injected into the cavity of the abdomen is DIRECTLY AND POWERFULLY SEDATIVE (both the voluntary and involuntary motions yielding to its influence) it neceſſarily follows, that the *increased redness* and *apparent inflammation* † could not have been the conſequence of the opium having acted as a ſtimulant, unleſs we ſuppoſe that it acts as a ſtimulant and a ſedative *at the same time*, which would be abſurd; and, indeed, Dr. Crumpe has ſhewn that idea to be unfounded.‡

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* Treatiſe on Poiſons, Vol. ii. p. 363. (Skinner's tranſlation.)

† Theſe appearances ſeem not to have occurred in Mr. Ramſay's experiment. Dr. Whytt ſays, nothing remarkable was ſeen in the abdomen; only, although it was opened ten minutes (20 minutes had elapſed in Dr. Crumpe's experiment) after making the injection, the inteſtines had no motion.

‡ "It is in the firſt place to be remarked, that opium cannot be ſeparated into any two principles, endowed ſolely with the oppoſite qualities of a ſtimulant and ſedative, as will be ſeen by recurring to the chapter in which

As they cannot be explained therefore by ſuppoſing the modus operandi of opium to be ſtimulant, it may be worth while to enquire, whether they do not admit of a ſatisfactory explanation, by ſuppoſing it to act as a ſedative.

We have ſeen, *that opium destroys the muscular power of the coats of the blood vessels*, though it is *not* applied *immediately* to their coats, but injected into a cavity, between which and theſe veſſels, a denſe membrane of cellular ſubſtance is interpoſed.*

It therefore follows, from the unequivocal manner in which this medicine acts, that this effect will be produced in a greater degree, when the ſolution is applied *immediately*, or nearly ſo, to their coats, as it is to thoſe of the peritoneum when injected into the cavity of the abdomen; conſequently, the veſſels having loſt their *tone* as well as their *action*, they will appear enlarged, and more red than uſual: Veſſels which were before inviſible to the naked eye, will now be readily diſtinguiſhed, and the parts may thus aſſume an inflamed appearance, ſimilar to what takes place when an aqueous ſolution of opium is applied to the tunica ſclerotica of a ſound eye, or to the cutis, recently deprived of the cuticula.

The above explanation was ſuggeſted by the experiments which have been adduced to prove, that opium injected into the cavity of the abdomen does *not* act as a ſtimulant: It has one advantage, (a negative one to be ſure) *it is not at variance with established facts*; but however unſatisfactory it may be deemed, that will not tend to invalidate either the experiments or the arguments which have been employed.

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which its analyſis is contained. The principal argument on which thoſe who attribute to this medicine both ſtimulant and ſedative qualities rely, is deduced from its being found firſt to increaſe, and afterwards diminiſh the frequency of the pulſe: But, as will be hereafter ſhown, this effect can be explained in a much more ſatisfactory manner, ſo that there is no neceſſity of reſorting in this caſe to a ſuppoſition, rendered highly improbable from conſidering, that we know of no ſimple in the whole Materia Medica poſſeſſed of powers in their nature oppoſite, and operating, when it is exhibited, ſeparately and diſtinctly. But even granting that opium is endowed with theſe contrary qualities, it will be difficult to explain on ſuch data the conſequences of its exhibition; for as the effects of ſuch principles are ſuppoſed to be diametrically oppoſite, it naturally follows that one muſt counteract the other; and hence, if both exiſt in equal proportions, the medicine would turn out perfectly inert; if one preponderates, it will act ſolely in conſequence of the exceſs of that principle; and to it alone, whether ſtimulant or ſedative, will all its effects be aſcribable." Inquiry, p. 167, 168.

* Wilſon's Eſſay on Opium, p. 60—63.

EXP. II.* "I raised the sternum and laid bare the contents of the thorax of a dog which had been just hanged; the heart was still contracting, but in a very irregular manner. I opened the pericardium and waited till all contraction ceased, and then let fall on it a few drops of the aqueous solution, milk warm. The contractions were instantly renewed and continued for about a minute, and the surface of the heart became unusually red. Upon the contractions ceasing, they were again renewed, but for a short time, by a similar application: A third attempt was ineffectual; but on injecting a small quantity into the superior venous sinus, the whole heart was again thrown into action, which continued for a short time, and was the last effort it could be roused to. I made a similar experiment with plain warm water. Its first application produced a slight renewal of the heart's contractions, the second and third were ineffectual."

Here some other cause must have been applied, that alleged being inadequate to the effects produced.

"I opened the pericardium (says Dr. Crumpe) and waited till all contraction ceased, and then *let fall* on it *a few drops* of the aqueous solution, milk warm. The contractions were *instantly* renewed, &c."

Now it is a well-known fact, that the shock occasioned by allowing any liquid to *fall* upon a part possessing sensibility is a powerful *stimulus*; besides, is it probable that a *few drops* of a solution of opium, containing *five-eighths of a grain to ten drops of water*,† could produce *any sensible effect*, applied to the external surface of the heart of a dog, in the situation here described, (whatever may be the modus operandi) *if applied without the addition of mechanical force?*

"Upon the contractions ceasing, they were again renewed, but for a short time, by a similar application: A third attempt was ineffectual; *but on injecting a small quantity into the superior venous sinus, the whole heart was again thrown into action*, which continued for a short time, and was the last effort it could be *roused* to."

I shall be able to adduce the most positive and unequivocal proofs, that an aqueous solution of opium applied to the internal surface of the hearts of frogs, *(no mechanical force being superadded)* INSTANTLY *puts a stop to their contractions.*

Dr.

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* Inquiry, p. 55, 56.

† "This solution, and that used in all the succeeding experiments, was made by dissolving opium in warm water, in the proportion of one drachm of the former to two ounces of the latter." Inquiry, p. 24.

Dr. Crumpe adds, "I made a similar experiment with plain warm water. *Its first application produced a slight renewal of the heart's contractions*; the second and third were ineffectual."

And the inference he draws from this experiment is, that "*Dropped* on the hearts of animals, *it accelerates their motions or rouses them into action if they have previously ceased to vibrate.*" P. 169.

And yet the only inference that can be drawn from the 30th experiment, (p. 129, 130) and which Dr. Crumpe evidently intends his readers to draw from it, is, that opium *diminishes the irritability and power of motion* in the hearts of frogs, when applied to them immediately after their separation from the body. But I shall give the experiment and his arguments, verbatim.

Fontana concludes from experiments related in his Supplement, that, "The hearts of animals taken from the body, and consequently deprived of blood, when immersed in watery solutions of opium, are *not* deprived of *irritability* and *the power of motion*, sooner than hearts immersed in pure water."*

"This fact (says Dr. Crumpe) I am under the necessity of denying, although the learned Haller is of the opinion, as well as the Abbé Fontana. Whytt asserts, from the result of seven experiments which he particularly details, that the *irritability and motive faculty* of the heart is, *as well as that of other muscles*, DESTROYED by opium.† In determining this point, it is to be observed, that if the hearts of a number of frogs, the animals on which such experiments are usually made, be separated from the body, though no application whatever be made to any of them, some will die, or lose their irritability, many minutes before others. It is therefore only from many experiments, and from taking a general view of the whole, that we can determine the point in question; and after having tried the experiment upon a great number of hearts, I do not hesitate to declare, that the *irritability* of that organ is *destroyed* by opium, *long before it would naturally disappear*. I think I have in general observed, that the larger the animal, the longer does its heart, when separated from the body, continue to vibrate; and in comparing the effects of plain water and opium, we

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* It has been fully ascertained by experiments made both before and since, that this conclusion of Fontana's is erroneous; but this very circumstance (as must by this time appear, and will be shewn more fully afterwards) throws another insurmountable difficulty into the way of Doctor Crumpe's Theory.

† "Essays Ph. & Lit. V. ii. Art. xx."

we ſhould, as nearly as poſſible, match the hearts employed in ſize. The liquors ſhould alſo be of the ſame temperatures, as a variation in this point will occaſion a manifeſt difference.

"With theſe precautions in view were the experiments above-mentioned conducted. The following are the particular reſults of the laſt inſtituted on the ſubject."

Exp. 30. "Having taken three frogs, one of a very large, another of the middle, and the third of rather a ſmall ſize, I ſeparated the hearts from their bodies, and immerſed them in pure water. The firſt lived *forty five* minutes; the ſecond and third *forty* minutes. Immerſing the hearts of three frogs, of nearly equal ſizes, in a watery ſolution of opium; the firſt lived but *twenty* minutes, the ſecond *eleven*, the third ſcarcely *ten*." Inquiry, p. 127, 130.

After what has been advanced, it muſt appear ſufficiently evident, that on the preſent occaſion there is a want of conſiſtency in Dr. Crumpe's arguments; and the following experiments ſhew, in a clear and deciſive manner, how far his 11th experiment is capable either of elucidating the ſubject of the modus operandi of opium, or of ſupporting the theory for which Dr. C. contends.

"It remains to examine (ſays Dr. Monro) how far animals are affected by the abſorption of opium, and its mixture with their circulated blood.

"But previous to this inquiry, I thought it might be of uſe to know what effect opium has when applied to the heart and vaſcular ſyſtem.

(Four Trials.)

I therefore laid bare the heart, and then injected into a large vein, which runs along the under middle part of the abdomen on the outer ſide of the peritoneum, a few drops of the ſolution,* viz. twelve drops into that vein of two frogs, and ſix into that of two others, and obſerved, that *as ſoon as the ſolution had entered the ventricle of the heart, that organ was rendered incapable of expelling its contents; and, in much leſs than one minute thereafter, became ſo entirely paralytic as not to make the leaſt contraction on the ſtrongest irritation, whether applied to its outer or inner part*; whilſt, at the ſame time, the extremities were agitated inceſſantly with convulſions, which continued for about three-quarters of an hour, gradually becoming more and more feeble; *nor was the animal able to perform any voluntary motion during this whole period.*

"Remarks.

* Conſiſting of a drachm of opium to an ounce of water.

" Remarks.

" 1. When we compare that part of the foregoing experiment which relates to the heart, with ſome experiments made by Dr. Whytt, when the opium was applied to its outer part, we ſee how greatly the delicacy of feeling of the inner ſide of the heart exceeds that of its outer ſide.

" 2. We learn that the heart is ſo far from being exempted from the influence of opium, as the learned Dr. Haller ſuppoſes, that, perhaps, *no muscle of the body can by any means be proved to be so much under its dominion as the heart, when the opium is brought into contact with its inner side*; in which way it will be applied if abſorbed."*

The following experiments are equally applicable to the preſent purpoſe.

" After ſecuring the aorta by ligature in twelve frogs of different ſizes, a few drops of the ſtrong ſolution of opium was injected into the heart of each. *It immediately ceased moving.* This, however, was not followed by the ſlighteſt convulſion in any part of the body.

" The frogs all died in exactly the ſame manner as theſe animals do when the heart is cut out; that is, when the circulation is interrupted, and when they loſe as much blood as in this experiment.

" I could not perceive that the injection of the ſolution had any effect, *but that of putting a stop to the motion of the heart.*

" A ſimilar experiment was made in the following manner: After dividing the aorta in ten frogs, moſt of them full grown, a few drops of a ſolution of opium, (not quite ſo ſtrong as that uſed in the laſt experiment) were thrown into the heart of each. Its contractions instantly ceased, but no convulſions ſupervened."

I ſhall paſs over the remarks which immediately ſucceed the above experiments.

" This, however, ſuggeſted another mode of making the experiment, which ſeems perfectly concluſive.†

" I ſlit the heart in ſix frogs. Notwithſtanding its contents were thus inſtantly evacuated, *it continued to contract with vigour.* A little of the ſame ſolution uſed in the laſt experiment was then dropped into it.

" No

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* Eſſays, Ph. & Lit. Vol. iii. p. 330—332.

† Theſe experiments were made with a view to aſcertain how far opium, applied to the heart, is capable of affecting any diſtant part through the medium of the nervous ſyſtem.

"No part of the solution, applied in this way, can be sent through the arteries to the brain; but as all the nerves are left entire and uncompressed, if the convulsions which follow the injection of opium into the heart depend on any action of that drug on the nervous system, they ought to be observed in this experiment.

"On dropping the solution into the heart, *it immediately ceased moving*, but no convulsions supervened."†

To attempt any comments upon the above important and decisive experiments, would only weaken their force; I shall, therefore, only add, that it is *fully ascertained by thirty-two experiments, that opium, applied to the hearts of animals,* IMMEDIATELY PUTS A STOP TO THEIR MOTIONS; *and it is also ascertained, on the authority of four experiments, that they become so entirely paralytic in less than one minute thereafter, as not to make the least contraction on the strongest irritation, whether applied to their inner or outer part.*

"It has been shewn (says Dr. Wilson) that opium, applied *to the external surface of the heart, very little, if at all, affects the muscular power of this organ*; applied in considerable quantity to its *internal surface, it immediately destroys it.*"‡

Dr. Whytt's experiments upon the hearts of frogs, *separated from the body*, and immersed in a solution of opium, shew very clearly that opium directly *diminishes* their mobility, sensibility, and irritability; but the fact is so generally known and acknowledged, that I shall pass on to his conclusion, p. 335.

"It appears, beyond doubt, from the preceding experiments, that the heart is not exempted from the power of opium, as the learned Dr. Haller has affirmed,§ but has *its motion destroyed by it, as well as the other muscles, only not so soon.*"

EXP. 12.‖ "Having separated, by inflation, the skin and muscles of the lower extremities of a frog, I poured between them a quantity of the watery solution; both limbs were deprived of the power of motion in about nine minutes; the animal died in fifty-five minutes."

The plain and necessary inference suggested by this experiment, is diametrically opposite to the conclusion Dr. Crumpe endeavours to establish.

It is evidently his design to prove, that opium is a *stimulant*; and, as a proof of the *validity* of the doctrine, we find, that on pouring

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† An Essay on Opium by A. P. Wilson, M. D. p. 48—50, and 53, 54.
‡ Essay, p. 85.
§ Act. Gotting, Vol. ii, p. 147 and 154.
‖ Inquiry p. 56.

pouring a solution of opium between the skin and muscles of the lower extremities of a frog, *both limbs were deprived of the power of motion in about nine minutes.*

These, to be sure, are *strong proofs of a* STIMULANT OPERATION; and when several *other consequences* of injecting opium between the skin and muscles of frogs (which occurred to Dr. Monro) come to be added, without doubt they must appear in the aggregate perfectly convincing.

In the following experiments the effects of the opium were not confined to the limbs, but acted generally.

"EXP. 6. I next endeavoured to ascertain the effect of the opium on the muscles when immediately applied to, or in contact with them, and how far the nerves of the skin would suffer by applying the opium to its inner side.

"For this purpose, I cut a hole in the skin at the top of the thigh, and poured the opium into the cellular membrane, between the skin and muscles.

(THREE TRIALS.)

"I first poured forty drops of the solution under the skin of the left thigh and leg, having before hand broke, with a probe, a partition between these made by the cellular substance of the knee.

"I did not attend to the effects till half an hour thereafter, when I found that whole member paralytic, its toes and skin having *lost their sensibility*, and the muscles their motion. *The animal seemed much stupified*, and could scarcely move its body from the place by the help of the other hind extremity; *and the blood had ceased from motion in the small vessels of both feet; though on examining the heart, I found it still gave twenty-two regular, but feeble, strokes in the minute.* After ten minutes more, all the members of the body were extended and convulsed, and the animal was quite unable to move its body out of the place where it was laid, and soon after died."

(TWO TRIALS.)

"I then tried the same experiment with twenty drops only of the solution, and observed, that *in about a quarter of an hour that extremity was much weakened and less sensible*, and in five minutes more was not only *motionless and insensible, but the animal seemed to be much stupified and lay still*, unless when it was hurt, and after nearly the same time as those above, had its members extended and convulsed, and soon expired.

(TWO TRIALS.)

After that I tried the same experiment with *ten* drops only. After twenty minutes, *that leg seemed to be weaker, and in ten minutes more, its muscles had lost their power, and the toes had little*

little sensibility; and now the animal seemed to be a good deal stupified, and the heart of one of them examined gave only 25 *strokes in a minute.* An hour and a half after the beginning of the experiment, the toes seemed to have *quite lost their sensibility, and the muscles their motion; but the other parts were not convulsed,* and the animal jumped by the help of the other hind extremity. *And two days thereafter, this leg had recovered both its sense and motion,* and the animal seemed quite well, and continued so for fourteen days at least.

(Two Trials.)

"In the last place, I poured ten drops under the skin of the thigh only in one frog, and the like quantity under the skin of the leg only in another, without finding that either of these members lost their sense or motion, or that the rest of the body was observably affected."*

In the following experiment the modus operandi is very clearly illustrated. In this instance the effects of the opium are confined to the limb.

"Having adapted the web of a frog's foot to a microscope, I injected *eight or ten drops* of a solution, (nearly as strong as the stronger solution) under the skin of the leg.

"In a few seconds *the circulation became languid, and no motion could be perceived in some of the larger blood vessels. It* gradually *became more obscure in the rest, till, in the space of about two or three minutes after the injection of the opium,* it ceased altogether. Nor did this interruption of the circulation proceed from any general affection of the system, since the motion of the blood still continued in the other foot.

"This experiment was made three times in the same manner, and with the same result."†

It must appear extraordinary after perusing the 12th, and especially when we come to consider the 30th, 32d, 36th, and 42d of Dr. Crumpe's experiments, that he should have expressed himself in such *unqualified terms*, as he every where does, when speaking of the modus operandi of opium.

P. 168, he says, "The facts and arguments I am about to relate have induced me to conclude, that all its effects are the result of a *stimulant property alone.*" And yet, Dr. Crumpe is so far from denying the existence of sedatives, that he allows the consequences resulting from fourteen of his own experiments, to denote a *directly sedative operation*; as will be shewn

* Essays Ph. & Lit. v. 3, p. 309—312.
† Wilson's Essay on Opium, p. 61—63.

ſhewn when we come to conſider the 32d and the ſucceeding experiments. The reaſon will alſo then appear, which induced him to draw a concluſion that is *contradicted even by his own experiments*, and ſtill more pointedly *by a large body of indubitable evidence*, collected from various, and from the moſt reſpectable, ſources of intelligence.

Exp. 15.* "The pure reſin of Exp. 13, when rubbed between the fingers, had more of the peculiar ſmell of opium than the gum; when chewed, it had no peculiar bitterneſs or pungency. The gum of Exp. 14, had more the ſmell of extract of liquorice than opium; to the taſte it was extremely bitter and pungent."

I have introduced this Experiment for the purpoſe of remarking, that though the activity of opium reſides chiefly, if not entirely, in the reſinous part, as is proved by the 17th and 18th of Dr. Crumpe's Experiments; yet that it poſſeſſes no peculiar bitterneſs or *pungency*; and that the gum, when ſeparated from the reſin, though it is inert, or nearly ſo, compared with the reſin, yet to the taſte it is *extremely* bitter and *pungent.*

Exp. 17.† "Of the pure reſin, procured by the means mentioned in Exp. 13, I took two grains diſſolved in a very ſmall quantity of ſpirit of wine, at one in the afternoon, my pulſe beating 70 in a minute:

In	5	10	15	20	25	30	35	40	45	50	55	60	75	90	Min.
P. beat	78	78	80	78	78	76	70	72	66	65	63	65	63	66	

"In fifteen minutes, there was an evident increaſe in the fulneſs as well as frequency of my pulſe; in 60, perceived a pleaſing languor and drowſineſs; in 90, this was ſo much increaſed, that I lay on the bed and ſlept for an hour; on getting up found myſelf languid, very thirſty, and affected with a ſlight vertigo and lightneſs of my head; on taking a glaſs of Port wine all theſe ſymptoms were relieved, and I ate my dinner with as good an appetite as uſual."

The only mark of a ſtimulant operation, either in this, or in any of Dr. Crumpe's experiments, (or indeed in any experiment I have met with on the ſubject) which has an *unequivocal* claim to be conſidered as ſuch, is a ſlight and tranſitory increaſe in the frequency, ſtrength, and fulneſs of the pulſe. Now, did this effect *conſtantly* follow the internal uſe of opium, which is well known to be by no means the caſe, of what force or conſequence could it be of, in whatever point of view it might be conſidered, when contraſted with the many *clear and*

* Inq. p. 62. † Idem. p. 65.

 decisive

decisive marks of a SEDATIVE *operation, which* INVARIABLY *follow, whenever opium is taken in a sufficient dose to affect the system at large?*

Besides, it will be demonstrated (if it has not been done already) that in every *other* practicable mode of introducing it into the system of different animals, (except by injecting it into the skull, or into the spine) whether it be injected into the cavity of the abdomen; into the blood vessels; between the skin and muscles; into the rectum; applied immediately to the internal surface of the heart; or externally to the skin, *it* DIRECTLY DIMINISHES *the frequency and force of the heart's contractions, and produces immobility, and total insensibility* to stimuli of every kind; and these effects *(diametrically opposite to those produced by stimulants)* commence in some of these experiments in a few seconds, and in ALL of them speedily, after the introduction of the opium, and increase gradually or rapidly, and are temporary or permanent, according to the quantity of opium which has been introduced, or the ability of the animal to sustain its power.* And the introduction of the practice of applying opium externally by means of friction, has furnished us with a very strong argument against the stimulant theory, for in this mode of applying it, it is conveyed by the lymphatics to the thoracic duct, and being there mixed with the chyle and lymph, passes with them into the subclavian vein, and from thence to the heart, the brain, and every part of the body.

Thus, as soon as it quits the absorbent system of vessels, (where I apprehend it undergoes no material change, except dilution) it is almost immediately applied to the centre of the sanguiferous and nervous system; consequently, its genuine effects, whether stimulant or sedative, must infallibly appear; and there can be no difficulty in deciding whether it *increases* the mobility, and *excites* the motion of the nervous power; or *diminishes* the sensibility and irritability of the system, and *thereby the motions and powers of motion in it.*

A brief recapitulation of the result of two Experiments I lately made,† will place the matter in so conspicuous a light, as to prevent even the possibility of a mistake.

The

* Opium injected into the substance of the brain, or through the spine, in frogs, neither retards nor accelerates the action of the heart; the reason of which is obvious; in this way of applying it, it can only act upon the heart through the medium of the nervous system. See Wilson's Essay on Opium, p. 74—85.

† Medical and Physical Journal, Vol. VII. p. 136, 137; and Vol. VI. p. 478—486.

The first was made at 6 P. M.—Before the commencement of the friction, the pulse was 108, firm and regular. In ten minutes, when nearly half of the tinct. of opium was rubbed in, it was reduced to 88, and was soft. In twenty minutes it was reduced to 84, was still soft, and Mr. Hall (the subject of the experiment) was affected with drowsiness, complained of being cold, and his eyes had a dull heavy aspect (a common occurrence when opium is introduced by friction). In thirty minutes, he complained of being cold and drowsy; pulse 84, and soft. In forty minutes 84, but weaker; still cold and drowsy. In fifty minutes, pulse 72, feeble and small; countenance pale; coldness continues, and is accompanied with disagreeable sensations. In sixty minutes, p. 76, weak, and not quite regular; complains of great debility. In seventy minutes, p. 90, regular, and rather stronger. In ninety minutes, p. 92. In one hundred and twenty minutes, p. 96; still complains of being cold, and the disagreeable sensations did not go off till after supper, when his appetite was *better than usual*.

The second Experiment was made before he had breakfasted or used any exercise; the tinct. of opium was increased from two to three drachms, and the mixture was made warm before it was applied; the pulse was 95. In fifteen minutes, p. 86, soft, and easily compressed. In thirty minutes, p. 83, weak and soft; he complains of being very weak, cold, and shivery; countenance looks sickly, skin feels cold. In forty-five minutes, p. 83, soft; complains much of being cold all over, but particularly his feet; is shivery and very weak, spirits depressed, feels drowsy, eyes look heavy. In sixty minutes, p. 90, weak and small; back painful; complains of being affected with a greater degree, and a different kind, of coldness to any he ever experienced before. I now asked if he had felt exhilarated at all, either in this or the former experiment? He answered, No; and could not help smiling at the question, as indeed he had reason. He ate his breakfast with an *uncommon appetite*, and was soon relieved from every disagreeable sensation.

The efficacy of opium applied externally, having been demonstrated in a variety of morbid affections, and its having produced, in every instance, a series of consequences directly the reverse of stimulant, renders the above experiments, in my mind, perfectly conclusive; because, having only to pass through the lymphatic vessels in its way to the heart, it is much less adulterated than when taken internally; being then exposed to the action of the gastric juice in the stomach, and afterwards mixed with the grosser parts of the food, the bile, &c. It is therefore demonstrable, that when opium does accelerate the circulation, raise the spirits, or increase the heat of the body, these

theſe effects muſt proceed, either from its immediate and topical action as a ſedative upon the primæ viæ, or from ſome other *adventitious* circumſtance, and not from its operating as a ſtimulant.

As this is a point of primary importance, and may have ſome weight in enabling us to form a juſt deciſion, I ſhall beg leave to ſuggeſt a few remarks, which may perhaps aſſiſt in the inveſtigation.

Does not an aqueous ſolution of opium deſtroy the power of action, in all muſcles (not excepting the heart) to which it is immediately applied? *

Does it not produce the ſame effect upon the blood-veſſels of the lower extremities, when injected between the ſkin and muſcles? †

Does it not put a ſtop to the periſtaltic motion of the inteſtines, almoſt immediately on being injected into their cavity? ‡ And does it not operate in the ſame manner, but in a leſs degree, when injected per anum? §

Is there any eſſential difference in the ſtructure of the ſtomach and inteſtines? ‖

Has not the ſtomach a vermicular or periſtaltic motion as well as the inteſtines?

Can the functions of the one be affected independently of thoſe of the other?

If theſe queſtions be anſwered in the affirmative, no farther proofs can be required, to convince us, that a ſolution of opium applied to the internal ſurface of the ſtomach, muſt diminiſh the ſenſibility and irritability, and thereby the motions and powers of motion, not only in that organ, but in the ſyſtem at large; or in other words, that it muſt act in this, as in every other inſtance, directly as a ſedative.

This theory will be found conſiſtent with the experiments as well as with the obſervations of different writers; or may rather be conſidered as the natural reſult of both.

"The

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* Whytt's Eſſ. loc. cit. p. 323—325. Monro's Eſſ. loc. cit. p. 309—314, 330, 331. Wilſon's Eſſ. p. 48—50, 53, 54, 60, 85.

† Monro's Eſſ. p. 309, 310. Wilſon's Eſſ. p. 59—63.

‡ ——, "Applied to the external ſurface of the inteſtines, I could not be certain that it at all diminiſhed the periſtaltic motion; on injecting it into their cavity, they almoſt inſtantly became paralytic." And in a note, Dr. W. ſays, "From this effect of opium on the internal ſurface of the inteſtines, we are at no loſs to account for the coſtiveneſs which attends the uſe of this drug." Wilſon's Eſſ. p. 85, 86.

§ Monro's Eſſay, p. 314, 315.

‖ Except that the inteſtines are not furniſhed with a ſet of veſſels which ſecrete a liquor ſimilar to the gaſtric juice, and this difference is of no importance on the preſent occaſion.

"The virtues of opium, (says Dr. Alston) internally taken, *depend chiefly on its action or influence on the stomach.* I have often observed a violent tenesmus removed *in a moment* by a few drops of liquid laudanum, vomiting stopped, pain eased, yea and sleep procured the same way, *and almost as soon.*"†

Every medical practitioner can bear testimony to the truth of these observations.

"In presence of, and assisted by Mr. Robert Fullarton, a curious gentleman, and very dexterous in microscopical observations, (in August, 1733,). I conveyed through a small glass tube a few drops of a solution of opium in water into a frog's stomach, and putting the animal into a glass cylinder, adapted it so to a good microscope, that we had a distinct view of a part of the membrane betwixt the toes of its hinder foot, where the circulation of the blood may easily be seen. My design was, since I found opium killed frogs, to observe if there was any visible change made by it in the blood itself, or in its motion; neither of us could indeed see any alteration of the blood, as to its consistence, colour of the serum, magnitude, figure or colour of the red globules; *but we very distinctly saw a surprising diminution of the blood's velocity, for it did not move half so swiftly as it uses to do in these creatures. We alternately looked at it again and again, and in less than half an hour saw the velocity of the blood gradually increase, the uneasy frog recover its wonted vigour, and the blood its common celerity*; upon which we took out the paddock, put it in a bason of clean water, and allowed it half an hour to refresh itself; *then gave it another dose of opium, fixed it to the microscope* WITH ALL EXPEDITION, *and viewed it as before; the blood then moved yet slower than it did the first time, and its velocity gradually decreasing, at length it stagnated, first in the smaller then in the larger vessels*, and in about a quarter of an hour the animal expired. One thing was very observable all along, viz. that notwithstanding the diminished velocity of the blood, there was no sensible diminution of the frequency of the pulse; yea, when there was no circulation or progressive motion of the blood in this part, the pulse was visible by an undulatory motion; that is, *the blood returned as far back at every diastole of the heart as it was protruded by the preceding systole; this continued till the frog was quite dead, or at least appeared to be so.* When we had lost all hope of its recovery I opened it, and found nothing in its stomach but a clear mucus like a jelly, a little coloured with the opium, of which it was full; every thing else seemed perfectly

† Medical Essays, vol. v. part i. p. 165.

perfectly natural. *This experiment we frequently repeated, and it had always the same appearances and event.* The recovery, however, of one of the frogs, which for a considerable time seemed to be dead, is not to be omitted. My friend and I one evening killed, as above, a couple of frogs with opium; one of them, which was the strongest, I laid half in water on a tile, in the bottom of a water pot, that if it recovered it might sit either wet or dry, as it liked best; the other I left on the earth dry under a hedge. Next morning, when I returned to the garden, I found the one under the hedge dead as I left it, but the other in the water pot was alive and appeared to be in perfect health.

While we were thus employed, another thing occurred, which though foreign to the present subject, it may not be amiss to mention. One of the frogs we got for the above experiments, *had not the use of one of its hinder legs, which was of a pale reddish colour.* This made me desirous to observe by the microscope the circumstances of the circulation in this paralytic, *and apparently inflamed member; and we found that the red globules were entirely dissolved*; *that the blood-vessels were distended with a reddish homogeneous liquid, as if the part had been injected with a bloody water*; *and that neither sense nor motion remained in it.*"‡

In the beautiful experiment just related, though it was frequently repeated and always with the same appearances and event, we do not find the circulation was accelerated, after the solution was thrown into the stomach of the frog; but on the contrary, that Dr. Alston and Mr. F. very distinctly saw (from the first) *a surprising diminution of the blood's velocity*; they alternately looked at it again and again, *and in less than half an hour* (when the action of the opium was over,) *saw the velocity of the blood* GRADUALLY INCREASE, THE UNEASY FROG RECOVER ITS WONTED VIGOUR, AND THE BLOOD ITS COMMON CELERITY; and, (which compleats the illustration) after allowing the frog time to refresh itself, they gave it another dose of opium, fixed it to the microscope, *with all expedition,* (five or six minutes might probably suffice,) and viewed it as before; *the blood then moved yet slower than it did the first time, and its velocity gradually decreasing, at length it stagnated, first in the smaller, then in the larger vessels, and in about a quarter of an hour,* the animal expired."

Manchester, March 9, 1802. M. WARD.

[To be continued.]

‡ Medical Essays, vol. v. part i. p. 153—156.

To the Editors of the Medical and Physical Journal.

Gentlemen,

I HOPE you will indulge me with inserting in the Medical Journal, a few Remarks upon Dr. Mossman's *unmerited censure*, and some very plain Observations upon Dr. Langslow's Reply; with which I propose to take my leave of him as a Disputant, perfectly convinced in my own mind, that Controversy is impracticable and Refutation useless. I presume Dr. M. to many of your readers, may seem to betray a want of that candour, the absence of which he so feelingly laments in others; otherwise, it is hardly possible he should, in so unqualified a manner, so seriously and severely blame a man he knows nothing of; and with the most unwarrantable freedom declare, that "whatever my reasons are for *not consulting with Dr. Langslow*, the disclosure cannot do honour to my judgment or my feelings." If, as Dr. M. says, he gives full credit to my own representation, and upon that alone grounds his verdict, I would wish to ask, how he can form such a conclusion, or conceive that a fellow creature could possibly have suffered, when it is asserted that I did not propose to leave my patient till she was so far recovered that my assistance was no longer necessary? Beside which he must have observed, that at the request of the party, however repugnant to my feelings, I did consent to stay, and at the desire of the same gentleman attended twice afterwards. These are strong circumstances against the charge Dr. M. has so uncharitably pronounced; and I have reason to believe, if he has perused the subsequent numbers of the Medical Journal, he will have seen so many proofs of my Opponent's intemperate and contemptuous language, not only towards myself but to Messrs. Wilkinson, Atkinson, &c. that I trust he will be inclined to abate somewhat of that severity which he has manifested towards an unoffending stranger. After all, if Dr. M. doubts my objections to such a consultation to have had any reasonable foundation, and though unacquainted with my character, declares my conduct is not to be justified; I must appeal from such a judgment to the justice of those better able to determine,—to the professional gentlemen in my own county and neighbourhood, and to those who have known me intimately for a long series of years.

I am now to notice some contradictions of Dr. L. upon extravasation or effusion, which he contended was the cause of *that particular case of Apoplexy*, the subject of controversy between us; and which he so *positively* asserted, was invariably present

present in every attack of real Apoplexy. In *explanation*, however, he says, the two words, extravasation and blood were not made use of by him, because the parties who were present assert they did not hear them; and still more wonderful to relate, because of this circumstance, Dr. L. says, "*the expression being directly repugnant in its import to my opinion.*" I think it wholly unnecessary to trouble the recollection of the parties for evidence which the Doctor himself has afforded me, and which I hope will enable us to establish the truth beyond all argument.

On the 28th of August (two days after the fit) the Doctor says, "We found the patient much better, and without any symptoms of *fresh effusion*, and the pain and oppression of the head even much abated, and I had reason to believe, the principal part of the *effused* fluid was *now* re-absorbed." If the effusion had not at first been asserted to be *considerable*, how are we to reconcile the principal part being absorbed, and *now* but little remaining.

Again "Mr. C. approved of this doctrine, (meaning the leeches) but denied *the extravasation.*"

It is difficult to imagine or to explain how that could be denied, which had not been affirmed.

Again, "I believe I did not say *considerable*, though if I did, the symptoms which Mr. Primrose told me existed *till after she was bled*, were fully equal to support *such an opinion.*"

This surely is not only acknowledging, that he said *extravasation of blood upon the brain in this particular case*, but defending the opinion of its being *considerable*, if Mr. P. was correct; of which there never was any reason to dispute.

I do not mean to animadvert upon Dr. L's physical romance, which he has addressed to Mr. Atkinson, or his strictures upon Mr. Wilkinson; those gentlemen, if they think them worthy of notice, will, I doubt not, correct the Doctor's errors, and fully refute his unfounded assertions.

An admired writer, says, "Surely the greatest arrogance that ever entered into the human heart, is that which not only pretends to be *positive* itself, in points wherein the best and wisest have disagreed; but looks down with all the insolent superiority of contemptuous pity on those, whose *impartial reasonings* have led them into opposite conclusions."

I am, &c.

Beccles, March 7, 1802. W. CROWFOOT.

To

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

WE must ever be amused with the seducing allurements of theory; and every practitioner embraces his own favorite hypothesis with all the glowing warmth of enthusiastic devotion. If the works of authors had been less exuberant in fanciful ideas, there certainly would have followed more practical advantages; but the labour of study is agreeably relieved, when an enlightened mind enjoys an expansive and philosophic view of the various and interesting phænomena of Nature. The illustrious names of Cullen and Darwin will be long remembered, and their works read with delight by succeeding generations.

The Controversy on Apoplexy is yet undecided. The doctrines of Dr. Langslow I have answered.* His defence against Mr. Crowfoot appeared to me, with some exceptions, to be pregnant with utility and just discrimination: But as he seems dissatisfied with my opinions, and as the contested points between us do not influence any material variation in the mode of treatment of the disease in question, I shall be brief in the subsequent observations.

I still contend, that "There often occurs Apoplexy without an effusion, or exudation of *blood, or* SERUM, upon the brain; and that when there is *considerable* extravasation, the case is commonly fatal." I now speak in general terms of the disease, without adverting particularly to the case related by Dr. Langslow. Notwithstanding the absorbents of the brain have not yet been discovered, their existence, from analogy, is generally believed; consequently they must be very minute, and in proportion to their magnitude must be the absorbing power. The absorbents in other parts of the system are comparatively large, and thus admit of a greater quantity of any effusion or exudation being absorbed in a given time. The blood, when extravasated, undergoes a chemical change before it is sufficiently fluid to be taken up by the lymphatic absorbents (*Zoonom. vol.* 3), and this must *considerably impede* the recovery of the patient. The *immediate* alleviation of all the formidable symptoms of Apoplexy, is easily explained on the principle of a turgescency of the arterial or vascular system of the brain; and this local plethora is uniformly remedied by blisters, cathartics, and venæsection. In fatal cases of extravasation, I conceive that the mouths of the absorbing vessels have been choak-

* Medical Journal, Vol. vii. p. 151.

ed up with coagulated blood, which acting as an extraneous body, produces inflammation, and ultimately, death. The causes of death, and of disease, are never allowed to be the same, and I accord with the sentiments of the ingenious Dr. Moffman, that "A sudden effusion of any *large quantity* of fluid upon the brain, is incompatible with animal life." Dr. Langslow does not coincide with Dr. Cullen, that expansion, congestion, or a fulness of the vessels in the head, will produce *compleat* Apoplexy, which I consider to be the most common cause of the disease, even in its most tremendous form. I recollect, when in Edinburgh, seeing a beautiful young lady fall down in an apoplectic fit in the street. Her breathing was loud and stertorous; her eyes were raised and glassy, but stedfastly fixed; and her frame was once or twice for a moment agitated.* It is probable that she might have recovered without any medical aid, as the violence of the paroxysms was greatly diminished before the usual means could be employed. It was in the morning when she was attacked by the fit, and in the evening the surgeon informed me, she was perfectly well. Thus I can hardly believe there was, in this instance, extravasation, exudation, or effusion. Lately I have been favoured with a case, by an eminent physician in this neighbourhood, where paralysis preceded the apoplectic seizure, and which clearly evinces that its fatality was caused by extravasation of blood upon the brain.

R. H. a strong healthy-looking man, after being exhausted with exertion, had profuse sweats, and his limbs affected with incessant trembling. At length the paralysis became universal, which continued two hours, and then he fell down suddenly in a fit of Apoplexy. He had every assistance the most skilful of the Faculty could administer, but death, in defiance of all their efforts, soon closed the scene. Leave could not be obtained to open the body, but a great quantity of blood was observed to have burst from his nose, ears, and mouth. Now I believe† palsy does not very often *precede* Apoplexy; but the eloquent and indefatigable Dr. Darwin infers that Apoplexy may be termed an universal palsy, or a permanent sleep (Zoonom. Vol. 4).

On

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* Are, frothing at the mouth, spasms of the muscular system, &c. &c. as represented by Mr. Wilkinson, (Vol. vii. p. 143) the diagnostic symptoms of Apoplexy? Are not they decisively characteristic of Epilepsy?

† I have known a severe hemicrania succeeded by a temporary paralysis. Opium was given in a sufficient dose, so as to produce intoxication; soon after the patient had a few hours long-denied sleep, and in the morning the shooting vertiginous pain was gone.

On the extraordinary doctrines of Mr. Crowfoot, a few words may suffice. Suppose we, for a moment, allow that the affection of the stomach is the predisponent cause of nervous Apoplexy! And that the distension of that viscus, from a full meal, compressing the abdominal aorta, occasions vertigo, stricture, and oppression of the head, so that the patient drops down senseless to the ground! And that during the exacerbations, an emetic is exhibited! What follows? The increased stimulation of the emetic will throw the stomach into more violent action, and the compressing power on the aorta will be, for a while, more forcibly applied. The vessels of the head being previously turgid and distended with accumulated blood; what will be the consequence of the additional impetus from this operation? It is true, the sensorial power is much diminished after the vomiting has ceased, but I regard it as a most objectionable and very injudicious remedy. If Mr. Crowfoot contends that there is no *pressure upon the brain*, I cannot imagine that the case is either nervous, sanguineous, or serous *Apoplexy*: For, certainly, no case of *real* Apoplexy *ever occurred* but was caused by an affection of the brain.

The theories of the most fatal maladies have been extremely multifarious. In the infancy of medicine, books were crowded with conjectures. The ancient professors, however, were uncommonly industrious. Dissections of the human body were then rarely performed; and although they might not be all men of well cultivated, luxuriant genius, they possessed, with their mechanical notions of organized matter, much sagacity and penetration. In a voluminous work, entitled, A Description of the Body of Man, published in 1631, I observe the following *very eccentric* Remarks on Apoplexy, with which I beg to conclude this Communication.

"I know well that when the jugular veins and the carotidall arteries are obstructed, the *caros* Apoplexy and Lethargy doe follow; whence the carotidall artery is called lethargicall and apoplecticall; and Hippocrates useth to call that kind of Apoplexy, *intuception of the veines*! Epilepsy, and such like diseases, doe proceed by the obstructing or cimmering of the spirits, which happeneth when the beginning of the spinal marrow is choaked by an unnatural confluence of humours unto it. And this opinion is confirmed by the dissection of heads of such men as die apoplecticall, in the ventricles of whose brains there is not found any greater quantity of excrement than is commonly found in other men. Moreover, the causes of an apoplexy, epilepsy, or falling sicknesss, and the incubus or night mare, are by all physitians acknowledged to be, when as phlegme, or crasse and thicke wind, is retained in the ventricles, which stopping them up either wholly or for the most part, doe *strangle the*

spirits

spirits therein contained. And I reade in Hippocrates, as in the text of his *Booke de Glandulis*, that the Apoplexy is occasioned by *the corrosion of the braine.*" I am, &c.

March 9, 1802. JAMES ATKINSON.

Philo-Medicus, *on Medical Graduation.*

To the Editors of the Medical and Physical Journal.

Gentlemen,

I Have read with pleasure in your last Number, the answer of an Old Practitioner, and the remarks of Aristarchus, jun. The former alleges, I am "a *young physician* who wishes well to the character, but who knows too little of the *character*, and is somewhat too fast." I can assure my old friend, that *I wish well to the character*, but am only a *spectator*. I have neither an Edinburgh, Glasgow, nor St. Andrew's Degree, nor one from any where else; hence there need be no suspicions that what I have said is out of envy at any part of the Profession, because they are not "of the same College." I can assure him too, I am as much a friend to real worth as he can be. I am of the same opinion with him, that "it would be better for the world if *sterling merit* were more prized than NOMINAL HONOURS." That "a Degree will never make a Physician," is the very point I am wishing to establish. A Degree is the Certificate of being qualified, when properly obtained; but not the Qualification.

"Never tell me," says your Correspondent, "of the superlative honour of certain Degrees, until Professors shall require of every Candidate to give oath that he did not attend those gentlemen called *Grinders*." Is not a *Grinder* a different name for a private Teacher? If so, what is the difference, whether a man be instructed in public, in private, or in both, provided he acquire sufficient knowledge before he obtain his Degree? I appeal to the old Gentleman himself, if it be not better to attend for "three years and nine months," and during that time to obtain knowledge by public teaching, grinding, and all the other means usually employed in preparing students, and then to receive a Degree? Or, to begin by first practising for a few years, and after we have acquired a little knowledge at the expence of the lives of our fellow-creatures, to think of taking out

a St.

a St. ——'s Degree, to make us respectable Physicians? I hope for the honour of humanity, there are few of this opinion. If a man by the little learning he has got, by his common sense, and by carefully observing the operations of Nature, has in the course of a few years made himself a respectable practitioner, what greater honour can he expect from having a St. ——'s Degree? The principal thing necessary to the procuring of which, is to find a man who will carry the money and bring back the Degree. Thus, in place of *Grinders*, we find *Mail-Coachmen* and *Carriers* necessary aids towards Graduation.*

All the Gentlemen who have Degrees from the Colleges of St. —— and A——, are not equally liable to these censures; there are some very respectable characters, who have practised for many years, and who have had recourse to this method: They are, however, to blame; for although they may be sufficiently qualified, it is leaving an open door through which persons of a very different description may pass. I consider my friend, from what he has said, to be a person fully qualified to practise as a Physician, hence there was no impropriety in giving *him* a Degree: But what would he think, instead of being associated with "an active, steady young man, who has served an apprenticeship of five or six years, and who has seen a good practice during that time," to be associated with a lazy, self-conceited weaver, who has left his loom, served an apprenticeship of only a year or two with a man little better qualified than himself, and instead of being at College "three years and nine months," only *four* or *five* months, and afterwards to practise till he could muster up as much money as would purchase a Degree? Such characters, however odious they may appear, are to be found among my friends, Fellow Physicians!

The old Gentleman says, "he glories to tell that he has long practised under the banner of a Degree from the ancient and honourable College of St. ——." I believe him well; but men who do not reflect on the causes and consequences of their actions, often "glory in their shame." The cause of his taking a St. ——'s Degree he has not mentioned; but the consequences are evident. I suppose my friend to be a Gentleman of respectability, and one who really does honour to the profession. The world knows his Degree is from St. ——. Another man, who would be a disgrace to *any* profession, by some means or other, procures a Certificate, and obtains

* "Ridicule," says Dr. Blair, "will often succeed in reforming Vice and correcting Error, when Reason and Arguments have failed."

obtains a Degree. Now he is upon a footing with my friend, and the public as naturally confide in the one as in the other, since they are "both practising under the banners of Degrees from an ancient and honourable College!"

But if individuals are to blame for procuring such Degrees, what culpability are we to attach to a body of men who thus prostitute the honours of an *ancient and honourable College?* They may plead their poverty; but a man's poverty is seldom a sufficient excuse for deviating so far from the path of rectitude. This my good old friend will call *railing*; but he should remember the proverb, *Desperate diseases require desperate remedies.* Had I not used strong language, my communication might not have been taken notice of, and the end would have been lost. But now, since two champions have taken the field, there is little doubt of setting every thing in its proper light.

Aristarchus wishes to know if I have been refused *literary honours.* I see no reason for asking this question, and as little for answering it. To him it is certainly a matter of indifference whether I am physician, surgeon, apothecary, or nothing, since the "Animadversions of every medical *controvertist* should be directed to the *subject matter* only, without any regard to the *authors* of opinions in dispute." I have been charged with using harsh language, but this is merely an opinion. If the subject required it, harsh language became necessary; and my style would not have been appropriate had I not used it. What one gentleman considers as harsh, illiberal, and unbecoming, another declares to be "laudable, well timed, and ingenuous."* So there is no deciding in matters of this kind; every man speaks as he *feels.*

I am of opinion with you, that Controversy without *salt* will soon become insipid.† No man would think of writing upon a subject of controversy, or on one so notoriously hurtful to society, as that of granting Degrees to persons unqualified,‡ in the same style as he would write a love epistle. I could produce many instances where the most beneficial effects have resulted from controversy, when it was carried on in the most personal manner, but I shall satisfy myself with one or two. The Disputes which happened between Dr. Cullen and Dr. Brown, and between Dr. Hunter and Dr. Monro, were carried

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* Medical and Physical Journal, vol. vii. p. 281.

† Note, vol. vii. p. 261.

‡ Whatever may have been the practice formerly, we have reason to believe that at present they do not grant Degrees without due satisfaction respecting the qualifications and character of the recommenders, as well as the reommended. EDIT.

ried on with the utmoſt invective; yet, notwithſtanding, theſe controverſies gave riſe to theories and diſcuſſions which have done much to the improvement of medicine. I believe that many of the ingenious theories of theſe gentlemen were ſtarted more with a view to thwart their antagoniſts, than to improve the healing art. But be this as it may, they mutually gave riſe to more philoſophical reſearch than was formerly known in the medical world. The Letters of Junius were full of invective and perſonalities; but will any man diſpute but they were of much advantage to the country at the time they were written? Public grievances and abuſes are not to be removed by a ſervile attention to all the formalities of faſhion, but by bold detection and expoſure to the world.

There is nothing more detrimental to the advancement of ſcience than compleatly following cuſtoms and authorities. To doubt is the firſt ſtep towards improvement. For many hundred years the world was fettered by the cuſtoms and doctrines of the ancients; but no ſooner did men begin *to doubt*, than Knowledge ſpread like lightning over the Univerſe. I hold it neceſſary to the advancement of ſcience that there ſhould be a diverſity of opinions. Nothing is a greater ſtimulus to exertion than a perſonal diſpute. It ſhews that a man's opinions are taken notice of; and what was ſaid of the Iſraelites in Egypt may be ſaid of them, the more they are oppreſſed the more they multiply. The minds of the individuals themſelves are often warped with prejudice, but the cool ſpectator never fails to catch a ſpark of knowledge from the colliſion of jarring parties. I have read many diſputes in your valuable Journal; in ſome of them, things were ſaid which might well have been omitted, but from all I derived improvement.

By giving the preceding reflections a place in your Journal you will perhaps contribute to the removal of an abuſe which has long been complained of, and you will oblige,

Your's, &c.

Scotland,
March 9, 1802.

PHILO-MEDICUS.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

A Great deal has been both written and ſaid upon the ſubject of Quackery, and I am afraid it is not in my power to ſay or write any thing to prevent it. Yet, it appears to me, that there is one thing which greatly tends to its being general:

general: Do we not frequently obſerve in every country town, *This is the Patent Medicine Warehouse*; and, in ſome druggiſt's Advertiſements, *Phyſicians Preſcriptions faithfully prepared.* But, let me aſk, how are they prepared? Not by the maſter; but by a dirty boy, who is enough to ſicken the ſtomach of the pooreſt, and who perhaps knows no more about Latin preſcriptions than a chimney-ſweeper. The preſcription is made up at random, and the perſon diſguſted with the idea of its being ſo dirtily prepared. A paper is perhaps put into his hand, giving a detail of the wonderful cures performed by ſome patent medicine; from the druggiſts he runs to the Patent Medicine Warehouſe, which is perhaps at a bookſellers, where the article aſked for is wrapped up clean; and not ſeeing the proceſs of its making, he becomes a purchaſer; and by ſuch means, if the patient gets better, aſcribes it to the quack medicines, and the article gets into repute.

I think another cauſe of the increaſe of quack medicines, is from the phyſicians frequently ordering their *gratis* patients to take their preſcription to the druggiſts, as they will there get it made up cheap; perhaps half the directions are written in Engliſh, that the poor ignorant boy, or the maſter, who is ſometimes a grocer and tea-dealer, &c. ſhould underſtand them.

I have ſometimes been ſurpriſed at ſeeing a number of preſcriptions lying at a druggiſt's ſhop waiting to be made up, and which were for ſuperior people; and upon minute inquiry, have received an anſwer ſuch as this: Dr. —— ſaid I might take it to the druggiſt, it would be made up as cheap as at Mr. ——'s the apothecary. The regular bred apothecary is thus left out, who has a good education, and attended to chemiſtry, &c. whilſt the druggiſt's boy is to make up medicines of which he does not know the uſe of one ingredient, and ſometimes, by miſtakes, has occaſioned the death of the patient. I am likewiſe ſorry to ſee Quackery ſo much reſorted to by the great, who fly to Quack Doctors who cure the gout by hot bathing; we have hot bath doctors and cold bath doctors, all for one complaint!

Through the medium of your highly reſpectable Journal, I could wiſh to caution all phyſicians, ſurgeons, &c. from ſending their preſcriptions to druggiſts, but let them go to the regular apothecary; the medicine would be faithfully prepared; and it would be of ſervice to the patient, and be one means of leſſening the increaſed tendency of the common people to run after Quacks.

I do not doubt but you, Gentlemen, will think with me, in this age of Quackery, every caution ought to be uſed to prevent

vent its increase; "That it has increased, is increasing, and ought to be diminished."

If you think the above worthy a place in your Journal, at some future period will give you a longer Essay on the Dangers of Quackery. I am, &c.

Leeds, Feb. 27, 1802. ANTI-QUACKERY.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

I Am happy to find that, to the honour of the practitioners in town, the tendency of my second query in your Journal (Vol. VII. p. 260) is not comprehended by you. It will nevertheless, I am sorry to add, be fully understood by very many of your readers, who might not, a priori, be thought more likely to determine its allusion, did not their injured feelings bring it home to their understandings in a manner equally forcible and unequivocal. I am not, however, of that party; and might rather, as an interested being, applaud a practice which tends to increase the number of my own fees, did I not feel that individual interest should ever give place to public good. With this conviction ever paramount, I lend my feeble assistance to the advancement of liberal science. Yet, while I assume the privilege of censuring the conduct or writings of others, I am well aware that I am no less open to *their* criticism, to that of your *readers*, and *yourselves*. I feel that the language of my Query is too general and obscure, and beg to submit the following as a much more comprehensive and intelligible mode of putting a question, which will, I trust, be considered as of some importance.

Does it consist with the dignity, duty, or honesty of a Physician, to prescribe in characters, or under denominations *unknown to the regular practitioners*; and which characters, or denominations, are understood by the *Druggist* ALONE, who is deputed by such Physician as the *sole* compounder of his prescriptions?

I do not find reason to alter the remaining Queries. My objections are directed to the admission of *personal invective*, or *scurrile reflections*, against the *writer*, and contend for the claims of the *subject matter* independent of the *man*.

I truly admire, though I may not have the power to imitate, the Attic salt of the Athenians; yet I do not admit *invective* or

scurrility as forming that salt in any scientific discussion. It is true that my preceptor, Aristophanes, was a master of all the acumen, diffusion, and graces of the Attic eloquence; and although in his *Nubes* he treated the ablest philosopher of his day with comic ridicule, who afterwards died a martyr to the envy and aspersion of his cotemporaries Anytus, Melites, and Lycon, yet I hope your Journal will not be poisoned like Socrates, or your correspondents merit the comic ridicule of an Aristophanes, or either meet the fate of the Anti-Socratic faction. Be this as it may, *I* shall have the power of avoiding what I would consider as unbecoming, and shall not deprive myself of the opportunity of collecting the *gold* from a work so highly useful to the Profession, because it may have an occasional alloy of *copper*.

I am, &c.

March 4, 1802. ARISTARCHUS, Jun.

CRITICAL RETROSPECT

OF

MEDICAL AND PHYSICAL LITERATURE.

[FOREIGN AND DOMESTIC.]

Naturhistoriche Fragmente; i. e. Fragments or Essays of Natural History, by G. FISCHER, Professor and Librarian of the University of Mentz, Vol. I. with copper-plates, pp. 253, in 4to. Frankfort, 1801. Varrentrap and Wenner.

BY the present publication the author purposes to contribute materials towards improving that branch of Natural History which comprehends the internal structure of plants and animals, and which has by no means kept pace with what is commonly called Natural History, or with the knowledge of the external form of organic bodies. The author has, by his former publications, gained such credit, that we may justly expect from his abilities and zeal the most interesting contributions, with regard to so extensive and intricate a subject.

The present volume contains, 1. A memoir on the great store of life extended over the earth, and on the attempts of Naturalists for exploring it; read at his accession to the Professorship of Natural History in the University of Mentz. 2. On the method in comparative

parative anatomy and physiology; this is either comparative or monographical, that is to say, we may contemplate an animal, or any part of it, either by itself in all its respective functions, situations, &c. or compare it with another animal, and point out the differences and their chief characteristics. He adds here an account of the wood cuts, representing skeletons of animals, by Teodoro Filippo de Liagno; a great rarity of literature, which appeared at the beginning of the 17th century. The memoir is concluded with some remarks on the method of representing anatomical subjects by drawing. 3. On the Museum of Natural History at Paris, particularly on the Cabinet for Comparative Anatomy. 4. On the different forms of the skulls of apes, with original drawings. After having previously observed, that apes, as well as makis, do not properly deserve the name of *Quadrumana*, because their posterior extremities are real feet, only with long, more moveable toes, or rather, feet with fingers, which, however, widely differ from those of the anterior extremities; the author proceeds to examine the skulls of apes, particularly with respect to the lines of face drawn by Camper, which of late has been adopted by Mr. Cuvier as the principle of division in the extensive family of those animals. It is curious to remark, that the more the angle of face in apes approaches to that of men, the more analogy they also have with the human frame in other properties of organization; and, vice versa, the more this angle recedes from that of men, the less similar they are to men in other particulars of their structure. For instance, the *Orang-Outang* of *Camper* has an angle of face from 56° to 63°, which is near to that in man, whose angle of face is 70°, and this animal also comes nearer to the organization of man than any other of the family of apes, viz. by the two lobes of its liver, by the processus vermiformis of the intestinum cœcum, &c. This memoir contains besides, some other remarks on the external form of the skulls of apes, the internal structure of which he intends to examine in the next volume. 5. On the extraordinary minuteness of division in the blood vessels of the gills of fishes: it contains also some observations on the liver and air-receptacles of these animals, as organs contributing to respiration. 6. On the exhaling vessels of a new species of *Carthamus*. 7. On fossil palms found in the umber mines of Libra near Cologne. The petrified fruits discovered in these mines belong, according to Jussieu, Lamark, and Desfontaines, to a tree of the family of the palms. The first plate represents the skulls of the Orang Shimpanese, of the Coaita and Cynophalus; the air sac of an unknown fish called Macaibi by the Spanish naturalists, &c. the fossil palm fruits, &c. The second plate exhibits different skulls of apes, and the fourth that of the Pongo in natural size, after an original drawing by Camper. We cannot conclude the account of this work, without sincerely wishing the continuance of so interesting a publication.

Lexicon

Lexicon nosologicum, pollyglotton, omnium morborum, symptomatum, vitiorumque naturae et affectionum propria nomina decem diversis linguis explicata continens, auctore PH. A. NEMNICH, *Jur. Utr. Licent.* 1801, *thirty-eight sheets in folio. Hamburgh & Leipsic. Price one Louis d'or, or about* 17s.

THE composition of a Scientifical Dictionary is a work attended with greater difficulties than is generally imagined, as it not only requires the knowledge of the different languages, but also of the things which make the object of it. The author happily unites both these requisites, of which his former publications are sufficient pledges. The present Dictionary contains all the generic terms of diseases in Latin, German, Dutch, Danish, Swedish, English, French, Italian, Spanish, and Portuguese, but in an inverse order, i.e. at first the Latin words with the subordinate German, English, French, &c. &c. and then the German words first, with the Latin, English, &c. explanations, and so on through all the above languages. Very few words seem to have escaped the notice of our author, who is undoubtedly entitled to our acknowledgements for this laborious and useful undertaking.

C. W. Boekmann's Versuche uber das Verhalten des Phosphorus in Verschiedenen Gasarten; i. e. Experiments on the Action of Phosphorus in different Kinds of Gases; edited by Fr. Hildebrandt, Professor of Chemistry and Medicine at the University of Erlangen, with five engravings; 1800, pp. 342, in 8vo. Erlangen for Schubart, Price 1 rixd. 12 gr. or about 5s.

THE assertion of Mr. Goettling that phosphorus strongly shines in azotic gas, appeared too paradoxical to the adherents of Lavoisier's system, and too contradictory to their opinion of the nature of that gas, as not to have met with the greatest opposition on their part. The transactions, however, on this subject, seem not to have proved decisive on either side, as the antagonists of Mr. Goettling did not proceed very candidly when they asserted, that all azotic gas, in which the phosphorus happened not to shine, was pure azotic gas. The author of this publication, prepossessed by no prejudice, and merely actuated by truth and impartiality, undertook to decide the still doubtful question; in which attempt he appears to have as much succeeded as our present chemical knowledge will admit, by making for that purpose numerous experiments both with uncommon accuracy and skilfulness. A new light has been thrown on the above assertion, and at the same time that substance, so interesting to naturalists, examined in different other points of view. *Sect.* 1. *Experiments on the Action of Phosphorus in Azotic Gas.* It is extremely difficult, and almost impossible, to obtain an azotic gas which is entirely free from other substances as well as from oxygen, because the substances generally employed for separating the oxygen from the azotic gas, either leave themselves oxygen behind or are dissolved in the azotic gas; and it was owing to this circumstance that the question could not be

be decided, whether phosphorus really emits light in *pure* azotic gas? Thus the azotic gas obtained by the combustion of a sufficient quantity of phosphorus in atmospherical air is not destitute of oxygen, which appears from the diminution of its volume, perceived at mixing it with nitrous gas; hence the luminous appearance of phosophorus in this gas is justly ascribed to the remaining oxygen. It may likewise be suggested, that when phosphorus is put into vessels, in which a sufficient quantity of it has been burned for changing the air into azotic gas, it will not shine; but it is obvious, that the phosphoric vapours, dissolved in such a vessel, do here prevent its illumination. Sometimes the azotic gas is mixed with nitrous gas, which likewise obstructs the illumination. After repeated experiments the author succeeded at last in separating the oxygen entirely from the atmospherical air, by means of sulphuric alkali and the amalgama of lead, and thus to obtain a very pure azotic gas, whose volume was not in the least diminished when brought into contact with nitrous gas in Fontana's eudiometer, and in which no smell could be perceived. The result of the author's observations is, that in such a pure gas the phosphorus does evidently shine, and even stronger than in the atmospherical air, and, what is particularly remarkable, emits light in it at a very low temperature, viz. at 10° Reaumur, below the freezing point; whereas it ceases to shine in atmospherical air at 4°—5° Reaumur above the freezing point. Phosphorus is oxydated in azotic gas and changed into phosphoric acid, and the volume of the azotic gas is diminished by this oxydation of phosphorus. Sometimes it happens that the illumination does not proceed in this gas long enough to perceive that diminution, which however is owing to the vapours of the phosphoric acid and oxydated phosphor that are here evolved. In order to remove these vapours, the author has found out a particular contrivance, but which is not to be understood without a figure. These observations seem to give much credit to an hypothesis, which the editor intimates in the preface, namely, that azote is perhaps composed of oxygen and hydrogen, however inconsistent this idea may be with our present theory of gases and their respective combinations. But a very sensible thermometer, made for that purpose, did not in the least betray any heat at this illumination of phosphorus in pure azotic gas, whereas this was immediately perceived in atmospherical air or in azotic gas, to which a little oxygen had been previously added. It is, however, most probable, that the evolution of heat in azote proceeds so extremely slow, as not even to affect the most sensible instrument.

Sect. 2. Experiments on the Action of Phosphorus in Oxygen Gas. Mr. B. made the most accurate experiments on the absolute consumption of oxygen gas by phosphorus being burned in it, but he never succeeded in making it entirely disappear, whether it was prepared from the oxyd of manganese or of mercury; there always remained a residuum, which was indeed very little, not exceeding sometimes

ſometimes $\frac{1}{120}$. Phoſphorus does not ſhine in the pureſt oxygen gas at low temperatures: in a temperature ſomewhat higher than 19°, he ſaw the phoſphorus yield vapours, which by degrees became luminous, melted, and at laſt caught fire. Phoſphorus, however, is oxydated in oxygen gas, even at thoſe low temperatures in which it does not ſhine.

Sect. 3. Action of Phoſphorus in the reſt of the irreſpirable Gaſes. In all thoſe kinds of gaſes which we have hitherto become acquainted with, viz. in the carbonic and pure hydrogen gas, the carbonated, ſulphurated, phoſphorated hydrogen gas, in nitrous, ammoniacal, ſulphurous, muriatic and fluoric gas, the phoſphorus does neither fume nor ſhine, and even in the ſuper-oxygenated muriatic gas, in which it is ſo eaſily inflamed, it ſhines but a few moments before it begins to take fire, and only in the oxydated azotic gas it fumes and ſhines at the ſame time. It is, however, to be remarked, that all theſe gaſes ſhould be free from atmoſpherical air, in order to prevent the illumination of phoſphorus; which, being very difficult to contrive, may account for the contradictory obſervations of other naturaliſts. All kinds of gaſes diſſolve the phoſphorus into phoſphoreous vapours, which, when expoſed in glaſs veſſels to the action of ſun-light, depoſe it again either as a duſt or as cryſtals and vegetations of different form and colour, yellow, brown, or blackiſh; theſe are repreſented in the ſubjoined plates, together with the apparatus which the author made uſe of for his experiments.

Diſſertation ſur les Fièvres pernicieuſes ou ataiques intermittentes; par J. L. ALIBERT, *Medecin et Membre de pluſieurs Sociétés. An. viii. pp.* 139, *in* 8*vo. Paris, Richard, Caille, & Ravier.*

Although more than two years have elapſed ſince the appearance of this publication, yet we ſhould accuſe ourſelves of neglect were we to omit acquainting our readers with this production of Mr. Alibert, General Secretary of the Medical Society of Emulation at Paris, whoſe reputation as a man of learning is ſufficiently confirmed by former memoirs, ſcattered in ſundry medical periodical publications of France. The object of this work, although we poſſeſs the excellent obſervations of Torti, Morton, Werthoff, Senac, and Cleghorn, is here again illuſtrated with new caſes of irregular intermittent fevers, obſerved by Prof. Pinel, and by the author himſelf; a new claſſification is added, and inquiries made with reſpect to the aitiology of theſe dangerous intermittents, according to our improved knowledge of phyſiology and therapeutics. The claſſification which the author adopts, is nearly the ſame with that of Torti, except that he only admits varieties of this kind of fevers, whereas Torti takes them for proper ſpecies. According to this claſſification, there are ten varieties of the irregular intermitting fever. viz. 1. Irregular choleric or dyſenteric intermittent fever. 2. Hepatic or atrabilious. 3. Cardialgical, i. e. attended with violent cardialgies and unſucceſsful efforts of vomiting the contents of the

the ſtomach. 4. Diaphoretic, i. e. attended with profuſe ſweats. 5. Syncope, or attended with fainting fits. 6. Algide, i. e. freezing fever, with little ſucceeding heat. 7. Soporous. 8. Delirious. 9. which degenerates and becomes continued; and 10. Epidemic. The firſt ſeven varieties of this fever are taken from Torti, the ninth and tenth from Werthoff and Lanciſi, but the eighth has been inſerted from Mr. Alibert's own obſervations; every paroxyſm of this fever is attended with violent delirium, which comes on with the froſt. The ſecond ſection of this work is devoted to inquiries on the nature, diagnoſis, prognoſis, and treatment of theſe peculiar fevers; and in examining their proximate cauſe, the author very judiciouſly avails himſelf, according to Chauſſier, of the three principal faculties of life, that of motion, ſenſe, and the faculty of generating heat (motilité, ſenſibilité, and caloricité). He agrees with former authors in conſidering the exhalations of ſtanding water, marſhy grounds, bogs, particularly at night and during ſummer and autumn, as the remote cauſes of thoſe fevers. Bogs ſituated in high countries, chiefly when expoſed to north winds, ſeldom occaſions them. The phyſical properties of ſuch air, which is particularly apt to bring on theſe fevers, have not yet been ſufficiently examined; at leaſt, the eudiometrical obſervations, made for that purpoſe, have not proved ſatisfactory; and the author propoſes therefore a new inſtrument, which he calls hydro-eudiometer, by means of which we are likely to obtain more ſatisfactory reſults.

With reſpect to the treatment, he ſtates a ſeries of rules which we ought to follow for the cure of thoſe dangerous irregularities in the intermittent fever. He dwells particularly on the uſe of the bark, which he always recommends to give in ſubſtance, in a doſe from ſix drachms to one ounce, contrary to the common practice of French phyſicians: it ought always to be given during the apyrexy, or at leaſt at the decreaſe of the paroxyſm. We may alſo perform a cure in caſes of no imminent danger, by taking away the irregular character of the fever, and proceeding afterwards leſs vigorouſly.

Geneeskundig Magazijn; i. e. Medical Magazine, volumes firſt and ſecond, &c. 1801. Roulofswaert.

This Magazine or Journal is principally under the direction of Dr. Ontyd of the Hague. It is remarked, in the preface, that although Holland has long been the ſeat of Medical Knowledge, and has been rendered celebrated by the diſtinguiſhed abilities of a Boerhaave, Albinus, Camper, Van Doeveren, and many others; though it abounds with learned and able practitioners in the different branches of the healing art, yet it is deficient in that united zeal that diſtinguiſhes itſelf in other countries, and prompts the medical world to communicate occaſional obſervations and incidental diſcoveries to the public; by which both the ſcience and practice of medicine are ſo advantageouſly promoted. The Doctor, therefore, in connection with other medical gentlemen, has opened a repoſitory for that ſpecies of publication. It is not intended to publiſh this Magazine at ſtated periods, but as often as the quantity and

and importance of the articles communicated may render it worthy of attention.

The outlines of the plan are, 1. To insert such discoveries, observations, and experiments, which may be communicated by the established practitioners in Holland, and shall appear calculated to advance medical knowledge. 2. To collect the observations which may be made concerning the influence of different seasons of the year; the state of the atmosphere; situation of towns and villages; mode of living; different occupations, &c. &c. as they affect the health of the Inhabitants. 3. To communicate proposals, plans, memoirs, &c. either originating from the Agent of the National Institute, or transmitted to him, as far as they relate to the subject of medicine. 4. To give extracts, and make critical observations on medical publications, foreign and domestic. 5. The result of enquiries made by the National Agency, on births, marriages, deaths, &c. 6. To insert communications and brief accounts of medical writings published at home or abroad; under which are comprehended, *Inaugural Dissertations, Literary Intelligence, Programmas, &c. &c.*

There is every reason to expect that a publication of this nature will greatly contribute to the promotion of medical science; and we propose to avail ourselves of it, by inserting from time to time, such Extracts from it, as may prove acceptable to the English reader.

MEDICAL AND PHYSICAL INTELLIGENCE.

[FOREIGN AND DOMESTIC.]

To the Editors of the MEDICAL and PHYSICAL JOURNAL.

Gentlemen,

As I believe every unprejudiced mind must, at the present moment, be sufficiently convinced of the great utility of the Vaccine Inoculation, permit me, with all due deference to my much honoured brethren in the Profession, to propose, That as a plan of national remuneration to the valued Discoverer is now before a Committee of the House of Commons, that in order to give that Committee a more ample idea of its efficacy, there should be a general confirmation of the same by the voluntary testimonies of every friend to the Inoculation throughout the kingdom.

In order to accomplish this end, I beg leave to propose, that there be a Committee formed in every city and town throughout Great

Great Britain: That such Committee shall be composed of Medical Gentlemen only, who, laying aside all private differences, shall join to come forward to support and confirm so honourable a testimony: That such Committee shall draw up a Memorial of their belief of its efficacy, and subscribe the same with their names and places of abode; and that as many sincere friends to the Discovery reside at distances from large towns, notice shall be given to them by the Secretary of the nearest town, intimating to them the nature of the business; and that the Memorial will lay at a certain place for their signatures for a prescribed time: That these Memorials shall be transmitted, as early as possible, to the care of the Editors of the Medical and Physical Journal, (post paid) to be forwarded to Dr. JENNER.

And that while our Brethren in the country are thus laudably engaged, we may follow the example of the Medical Society of Guy's Hospital, by appointing a Committee to receive the signatures of those Gentlemen in London, and its vicinity, who are feelingly alive to the importance of the practice, of rescuing our fellow-creatures from the destructive and mutilating effects of that scourge to the human race, the Small-pox; a disease which, like a mighty torrent, has for many years past, swept away the best hopes of individuals in particular, and this nation in general. I humbly think, we need no other proof of the superiority of the Vaccine Inoculation than the Bills of Mortality for the preceding year, in which we see the number of deaths from the Small-pox were decreased from the number of the former year full 1000.

I hope I shall be believed when I say that I have not the smallest knowledge of Dr. Jenner, and that my motive in this arises only from a desire to the farther diffusion of so beneficent and important a Discovery, which has now triumphed over so much opposition; and that as I honour the Discovery, I desire, in conjunction with every well-wisher to his country and the community, that an adequate Reward should be given to the Discoverer.

I trust all your Readers will see this subject in its true light; and though some of them, perhaps, may think this Communication useless, I hope they will look upon it as it really is, only as a Hint which I wish may be improved upon by a more able pen than him who begs leave to subscribe himself,

Your's, &c.

J. C KNIGHT.

London, March 19, 1802.

The incalculable loss which this kingdom, and indeed the whole world, has sustained by the death of the Duke of Bedford, will naturally turn the attention of the Public to the important subject of HERNIA. The number of fatal Cases which comes to the knowledge of professional men, is sufficient to keep their attention always awake to the danger of neglecting any kind of Rupture, especially those which permit any considerable portion of intestine to

to protrude; for though a patient, or his ſurgeon, may replace the bowel with great eaſe a thouſand times, yet, in ſome fatal moment, as happened in this much-lamented inſtance, a ſudden contraction of the orifice may take place, and the reduction be rendered impoſſible without an operation. The danger of the operation, even in the moſt ſkilful hands, is ſufficiently known to make us anxious to endeavour to prevent the necessity of it by the uſe of proper Truſſes and Bandages.

To the EDITORS of the MEDICAL and PHYSICAL JOURNAL.

Gentlemen,

AS we have, at the particular deſire of ſeveral profeſſional men of the firſt reſpectability, avoided all the common modes by which our invention of a Truſs might have been more generally known to the public at large, we have to ſolicit that it may have a place in your valuable Publication, that we may, through ſo reſpectable a medium, have the opportunity of making it known to the Faculty in general.

Our Patent was obtained for the *art of bending tempered steel without the use of heat.* Upon ſubmitting this diſcovery to ſeveral Surgeons of the firſt eminence, they have given it as their individual opinions, after the moſt poſitive and repeated proofs of its operation under their immediate inſpection, that a Truſs made upon this principle is far ſuperior to any ever yet invented, both for *durability and effect,* beſides their having this peculiar advantage, that they are worn without ſtraps.

We have no doubt but they will be, when known, generally adopted; and we are ſanctioned in this opinion by having it in our power to ſay, that the Army Elaboratory now uſe them to the excluſion of all others; the Royal Hoſpitals of Greenwich and Chelſea, including out-penſioners, invalids, &c. the York Hoſpital, &c. entirely uſe them; the St. George's Hoſpital, the Mary-le-Bone Infirmary and Workhouſe; the Greenwich alſo, and other Inſtitutions, have adopted them. These are facts which will, the Patentees truſt, ſpeak loud in their behalf; however, it is the merit of the invention, and the judgment of thoſe who are competent to decide, that they wiſh ſhould recommend them to notice.

It may not be improper to mention, that the Patentees, who are gun-makers, No. 37, Charing Croſs, vend them entirely themſelves; nor do they make any ſhow of this article.

We are, &c. &c.

Feb. 24, 1802. TATHAM AND EGG.

Dr. PRIESTLEY has replied to Mr. CRUIKSHANK's Defence of his new Syſtem of Chemiſtry. He is ſtill the zealous Advocate for the doctrine of phlogiſton, and calls upon Mr. Cruikſhank to re-conſider his hypotheſis, and extend his examination to all the other arguments advanced in favour of the phlogiſtic ſyſtem, and againſt the decompoſition of water.

In

In another communication addreffed to Mr. NICHOLSON, Dr. [illegible]Y has detailed a number of obfervations and experiments [illegible]the Pile of Volta, which feem favourable to the hypo[illegible] electric fluids; the pofitive containing the principal [illegible] and the negative, that of phlogifton. Thefe united [illegible], conftitute the two kinds of air, viz. dephlogifticated and [illegible]able. He fays they tend likewife to confirm a conjecture [illegible]ced by himfelf many years ago, refpecting the fimilarity of [illegible]lectric matter and phlogifton, and, together with proper Galvanic experiments, fhew that the fame fubftance elaborated from the aliment by the brain, is the caufe of mufcular motion, the nerves being th[illegible] moft fenfible of all electr[illegible]eters.

In SCHERER's Che[illegible]ical Journal, Number 40, we have an analyfis of fome bituminous w[illegible], made by R. JAMESON, at Freiberg, who conceives that he has difcovered a new acid, which cannot be cryftallized, and which, by evaporation, is feen under the form of fmall fhells or fcale[illegible], of an acidulous tafte. It is difficult of folution when combined wi[illegible] lime; it decompofes all the nitrat and acetat of lead; it prod[illegible]es a brown precipitate in the fulphat of copper; in the fulphat of [illegible]on, the colour of the precipitate is of a deeper brown. The folution with the nitrat of copper takes a beautiful green colour, without any precipitation. It decompofes alfo the nitrat and muriat of barytes. This acid, mixed with a folution of indigo, in the fulphurous acid, produces a fine green colour. It refembles carbonic acid the moft, and, when poured on carbon, it forms a brown and bitter matter, which is foluble in water, fpirits of wine, and alkaline folutions. It then gives out a very penetrating and aromatic odour. The author thinks that the acid is compofed only of carbon and oxygen.

The Medical Repofitory of New York, contains the following dreadful picture of Irifh Emigration. Several fhips which had carried flax-feed to Ireland returned in the months of June and July to New York, crowded with needy and wretched emigrants from that ifland. They were fo thick between decks that the air was deprived of its ufual portion of oxygen, infomuch, that on bringing the fick paffengers to fhore, the common pure atmofphere was too ftimulant for their lungs, and a number of them gafped in it, and died in a fhort time. There was fo much animal excrement accumulated in one of the fhips, that the Health-officer detained her at the quarantine ground as poifonous and peftilential, and refufed to let her come up to the city. By the pukings and purgings, and by the urinary and perfpiratory difcharges of thefe miferable creatures, literally wallowing in their own filth, the bodies of many of them were befmeared and incrufted, forming a layer of excrementitious grime from head to foot. Their clothing and their bedding were impregnated with as much of thefe excrementitious matters as they could wipe from the bodies of the paffengers and abforb. And with fuch coverings, vile, offenfive, and peftilential in the higheft degree, were they furrounded. And thefe excrements, infefting every thing in the neighbourhood of the fick, underwent the ufual chemical

chemical changes in a heat nearly or quite equal to the hum[illegible] body, and turned to septic acid, or to some other septic and [illegible]sonous matter, which forms the exciting cause of feve[illegible] fever so excited, between thirty and forty from one [illegible] crossing the Atlantic, and were thrown overboard. T[illegible] arrived in a state of uncleanliness, sickness and want. [illegible] in America but among the emigrants from that unhappy c[illegible] who make so large a number of the poor in the American ho[illegible] and alms-houses. So thoroughly contaminated with their ow[illegible]rupting excretions were the clothes and beds of these sufferers, that the septic exhalations from them poisoned the air of the Marine Hospital, on Staten Island, and the medical attendants and nurses sickened in the discharge of their humane attentions. Mr. Bayley, the Health-officer of the port of New York, caused the sick, after landing, immediately to be distributed or separated from each other as widely as the circumstances would allow, that their pestilential exhalations might be diluted, and wafted off. He ordered their nasty clothing and bedding to be carried away from their persons, and that part of both which was too poisonous, ragged and rotten to be worth the cleaning, to be burned or thrown into the Bay. He directed the bodies of the sick to be purified by careful ablution and scrubbing with a solution of soap in water; and even the heads of some of them to be shaven. After these things were done, the sick were furnished with clean clothing and bedding from the public store. The walls of the hospital were repeatedly white-washed with lime, and the floors and utensils scrubbed with alkaline ley of potash.

So offensive and intolerable were many of these languishing creatures, that they were accommodated under large tents, for the benefit of more complete airing; and it was remarked, that the ground on which the tents were pitched grew too unsafe, in a few days, to be dwelt upon any longer, and the tents were removed, and erected upon fresh portions of earth. In such cases, the envenomed and deserted spot was regularly sprinkled over with lime. The Health-Officer, knowing that hard or bar soap was made of soda, and contained withal, as manufactured in New York, a large quantity of turpentine, which only added to its weight without increasing its virtue, procured for the use of the wash-house a stronger soap, made of the more powerful alkali, pot-ash, combined with just enough of animal fat to lessen its causticity so as to bear handling. With this very efficacious soap, and not with the common mixture of turpentine, flush and soda, in the shops, were the remaining clothes cleansed and alkalized. The consequence of this management was, that as soon as these regulations could be carried into effect, the pestilential vapours were dispersed through the surrounding atmosphere; a vivifying air was admitted into the lungs; the pestilential matter adhering to the bodies and clothes was alkalized and overcome, and poisonous effluvia issued from them no longer. It is worthy of being remembered, that Emigrants from Ireland, landing immediately in the city, instead of being detained at the Marine Hospital, filled New York with death and terror

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[illegible] The Editors of the Medical Repofitory, add to this [illegible] he following reflection: "The benevolent and philofo[illegible] emen of Ireland would be worthily employed in pre[illegible] e calamities, if poffible, among their countrymen, and [illegible] lieving the United States from fuch fhocking fcenes."

[illegible] other instance of pestilence engendered in a ship crowded with [illegible] ngers from Ireland, copied from a subsequent number of the Medical Repository. The fhip Nancy, Capt. J. Heron, was chartered by a commercial houfe at Sligo, to carry paffengers from that port to New York. She failed from Sligo on the 12th of July, 1801, and arrived, after a paffage of feventy-feven days, at the port of New York, on the [illegible]th of September following. This fhip, of the burthen of 202 tons, received on board 417 paffengers, and was navigated by nine feamen. The provifions, mere refufe, put up by Contractors with the view of faving expence, were of the worft kind; and the water, which was alfo of bad quality from the unexpected length of the voyage, became extremely fcanty before the arrival of the fhip. In order to receive fo great a number of paffengers on board this fhip, temporary cabins were built on the quarter deck, which were filled with eighty perfons, and three hundred were crowded into the fpace between decks. It will excite no furprife that a veffel, thus crowded, became fickly foon after failing from Sligo. Typhus fever and dyfentery foon began to prevail, and deftroyed the lives of a large proportion of the paffengers. In addition to the wretchednefs of being confined in fuch numbers in fo fmall a fpace, thefe unhappy Emigrants fuffered all the evils which their habits of uncleanlinefs could produce. Their bodies and clothes, covered and faturated with filth, exhaled poifon all around them. Partly from the want of ftrength and affiftance among the fick, and partly from the want of a fenfe of decency, the fpace between decks, occupied by nearly 300 perfons, became the receptacle of all excremental matters, infomuch that they iffued in fteams from the fcuppers. The filth on the upper deck was nearly over the fhoes. The fides of the fhip were daubed and incrufted with excrements; and even the ropes for the fupport of fuch as wifhed to go on board, were unfit to be handled. The ftench was intolerably offenfive. In fuch condition arrived this unfortunate veffel at the place affigned for quarantine in the port of New York. Ninety perfons had died on the paffage; one hundred and eighty were fick; fcarcely a healthy countenance was to be feen on board of the fhip; very few had efcaped difeafe, and many had fuffered from three to four relapfes. About forty were taken ill after their arrival. As foon as poffible the fick were brought on fhore, ftripped of their filthy and peftilential clothes, their bodies thoroughly wafhed and fcoured with foap and water, and then wrapped up in clean blankets, and carried into the wards appointed for their reception in the Marine Hofpital. The permanent buildings of the eftablifhment were infufficient to receive fo great a number; tents and other temporary accommodations were provided for the remainder. Separation, ventilation, and cleanlinefs,

cleanlinefs, as foon as they could be brought into action, acco[illegible]plifhed every thing that could be expected; and only [illegible] have died fince their arrival at this port.

Dr. BATTY's Lectures on Midwifery and on the [illegible] Women and Childr[illegible], [illegible]ill commence at his houfe in [illegible] Street, on Monday, April 12, at eleven o'clock in t[illegible]e morn[illegible]

Dr. DENNISON and Dr. SQUIRE will begin, early in the [illegible] month, a Courfe of Lectures on the Theory and Practice of Midwifery, and the Difeafes of [illegible]omen and Children. Notice of the day will be advertifed in the [illegible]blic [illegible]apers.

We underftand that Mr. Allen will begin [illegible] Courfe of Lectures on the Animal Economy at Edinburgh, [illegible] the beginning of May. In thefe lectures the phyfiology of the human bo[illegible]y is explained, and various pathological facts illuftrate[illegible], with all [illegible]at precifion and accuracy for which this gentleman is fo juftly and fo eminently diftinguifhed. We may remark, that Mr. All[illegible] has the merit of being one of the firft public teachers who in[illegible]roduced into notice thofe philofophical views of this branch o[illegible] kno[illegible]ledge, which are daily becoming more general, and which promife fo much advantage to the art of medicine, as well as to phyfical [illegible]cience.

On Wednefday, Feb. 24, Dr. BABINGTON, Affiftant Phyfician to Guy's Hofpital, was unanimoufly appointed by the Governors, Phyfician in Ordinary, in the room of Dr. SAUNDERS, refigned. And on Wednefday the 10th of March, came on the election of an Affiftant Phyfician, when Dr. JAMES CURRY, of Old Broadftreet, and formerly Phyfician to the Northampton Hofpital, was unanimoufly chofen to that office.

Shortly will be publifhed, Strictures upon the Obfervations of Mr. Crowfoot; with an Appendix, containing a Copy of Doctor Moffman's ingenious Paper on Apoplexy; together with Doctor Langflow's Anfwer to Meffrs. Wilkinfon and Atkinfon.

To CORRESPONDENTS.

The great influx of original and temporary matter, which always claims a preference, has obliged us to postpone several valuable Communications as well as books. Drs. Johnstone, Ferguson, Girdlestone, Woodforde, Bellamy, Vage; Mess. Mills, Watkinson, Philo-Medicus, Medicus, A. R. Hare, and Fowler, shall be noticed as soon as possible.

ERRATA.

Vol. VII. p. 275, l. 2, from the bottom, for *forgot* read *forgotten*.
p. 281, l. 2, from the bottom, for *agree* read *argue*.

Case of Monstrosity communicated by M. Cam

J. West del.